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The Magazine of New Energy Technology

Cold Fusion • New Energy

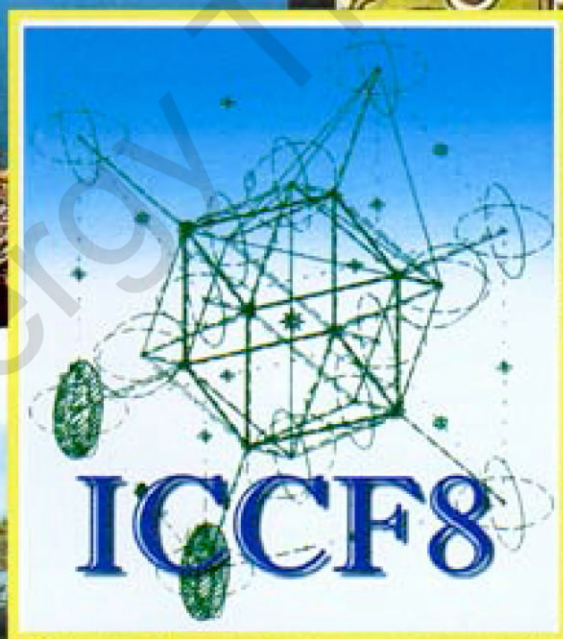


Renewable Energy

Volume 6, Issue 31 • 2000

A Critical Evaluation of the Pons-Fleischmann Effect

by Dr. Edmund Storms



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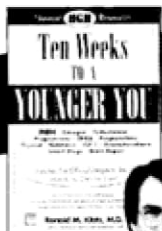
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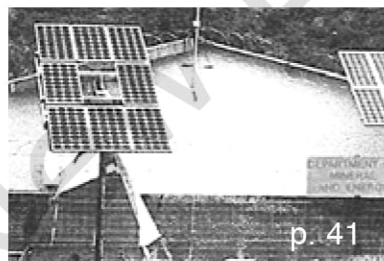
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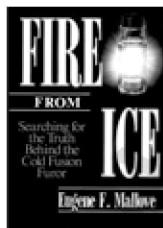
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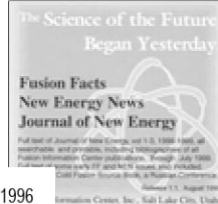
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Journal of New Energy: Jan. 1996-Fall 1998
Cold Fusion Source Book: May 1996, 313 pp.
New Energy News: Jan. 1997-Nov/Dec 1998

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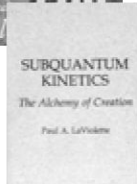
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Subquantum Kinetics: The Alchemy of Creation

Paul LaViolette, Starlane Publications, 1994
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BREAKING THROUGH

Welcome ICCF-8—Liberate Science!



by Eugene F. Mallove, Sc.D.



If you are among the several hundred fortunate enough to be reading this in Lerici, Italy during May 21-26, 2000, you find yourself at the Eighth International Conference on Cold Fusion—ICCF-8. Welcome! It seems that we have returned to the beginning, at least for this magazine. We are back in Europe. It was at ICCF-5 in Monte Carlo, Monaco, April 9-13, 1995, that some readers were introduced to *Infinite Energy*, when Issue No. 1 was distributed free-of-charge to attendees. ICCF-6 (Toya, Japan, 1996) and ICCF-7 (Vancouver, BC, 1998) followed the agreed Asia-North America-Europe rotation, and we continued the policy of complimentary issues at ICCF's. (For those many of you at ICCF-8 who are already subscribers, we are sure your extra copy would help to enlighten a friend back home!) Maybe it is a happy "omen" for our discussions in Italy that "8" represents a kind of vertically aligned infinity or möbius strip sign.

It has been eleven long and difficult years since the announcement by Drs. Fleischmann and Pons at the University of Utah in March 1989. It is appropriate at this time to take stock of where we have been, where we are, and where we are going.

It is gradually dawning on many in the cold fusion field that at least one of the claimed low-energy nuclear reactions in electrolytic and gas-phase systems has been significantly con-

firmed: that of heavy hydrogen fusion leading to helium-4 in and on the metallic palladium lattice plus commensurate thermal energy. The work at SRI International and at the U.S. Navy's laboratory at China Lake, California are fine confirmations, and other results reported at ICCF-8 may lend additional support. So, we can proudly say that the "miracle" of radiationless, deuteron fusion—cold fusion—is a reality. Recalling the eloquent aphorism of the late Nobel laureate Julian Schwinger, who was with us at ICCF-1 and at ICCF-4 (via a paper that I was privileged to read for him): "The circumstances of cold fusion are not those of hot fusion." This has been a very difficult concept for some critics to grasp, but it is the essence of the cold fusion conundrum. As Schwinger also emphasized in his talk in the fall of 1991 at MIT, "The excess energy liberated in cold fusion is not significantly transferred by radiation."

If there had been no other discoveries in this field, that alone should have caused bells to ring around the world to shouts: "Cold fusion is real!" They will ring eventually, when the world realizes what this means: Go to any pond, stream, lake, ocean, or snow bank and capture a gallon (3.79 liters) of water. In that gallon there is enough energy obtainable from the cold fusion of deuterium (heavy hydrogen) to helium-4 to equal the chemical combustion equivalent of about 300 gallons of gasoline. That is more than enough to drive even a heavy American automobile across the United States and back. Only a 1/6,700th of the hydrogen in that gallon would be converted to environmentally benign helium. As world leaders convened in recent times about increases in the price of crude oil, the inevitable insignificance of such concerns should have been the biggest issue on the table: how to plan for the breakout of the Water Fuel Age. Since no bells have been ringing in the general

media, these leaders are clueless about what the future will hold.

The primary confirmed cold fusion reaction is *far* from all that is on the table. With minute resources, compared to what governments and corporations squander each year on prosaic or failed energy research, the cold fusion/new energy community has discovered that the original Pons-Fleischmann system was but the tip of the iceberg of other energy processes derivative from water. Ordinary water electrochemical cells and ordinary hydrogen gas systems have been found to release stupendous energy, by some accounts at least 1,000 times the chemical combustion value of the hydrogen in the system.

A debate has raged since 1991 whether these ordinary hydrogen systems are giving rise to such mind-boggling occurrences as the low-energy transmutation of heavy elements, or whether catalytic shrinkage of the hydrogen below normal ground state (Randell Mills' "hydrino" formation) better explains the energy release. In my view, *both* these processes could be going on in different systems, or even simultaneously within one system. The data supporting both types of products—transmuted elements and hydrinos—are increasingly on stronger ground. The common theoretical framework that encompasses all the data is yet to be worked out. The very good news is that the "coming age of hydrogen power," ordinary chemical fuel cells and the like touted in many fine environmentally aware publications, will be at least 1,000 times more profound than most of them realize.

We still have the question of when all this joy and goodness is likely to happen. "Not soon enough!" is our stock answer, since we have been so disappointed by failures of several companies. What is *indisputably* holding the cold fusion/new energy field back is the lack of tangible embodiments of the science on the

desks and lab benches of entrepreneurs, as well as in college physics/chemistry labs. If some company or inventor leaps ahead with a utilitarian embodiment of a home heater or electric power generator, so much the better.

But even were such a reliable device to arrive tomorrow, it would have virtually no effect on the world if it were not immediately available for sale in large enough numbers to make the *tangible embodiment* of this science undeniable. The professional propagandists and bamboozlers who declaim against the "fraud" of cold fusion and new energy science—you know who they are—will continue to spread their Big Lies with impunity. Sorry to say, the peer-reviewed scientific literature of our field is just no match for their unholy alliance with most of the general science media. The day after the arrival of tangible embodiments, however, these conmen will be sent packing.

The two predictable accomplishments of cold fusion/new energy will be a huge expansion of scientific knowledge and a radical revolution in energy technology. But there is another possible outcome beyond these: a much-needed reformation and *liberation* of the process of science itself. Revolutionaries in other scientific fields are struggling toward that goal too. We share a common purpose.

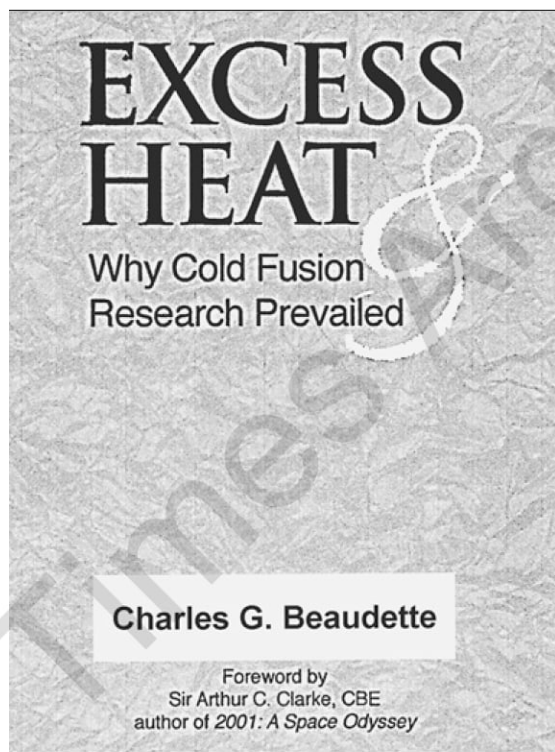
In this issue, I review the books by astronomer Halton Arp (p. 32), which deal with striking anomalous data that could be as revolutionary to cosmology and physics as cold fusion is to chemistry and physics. The process of data denial that Arp and his colleagues have faced is as pernicious as what has befallen cold fusion/new energy people. In *Seeing Red*, Arp writes: "Those friends of mine who also struggle to get the mainstream of astronomy back on track mostly feel that presenting evidence and championing new theories is sufficient to cause change, and that it is improper to criticize an enterprise to which they belong and value highly. I disagree, in that I think that if we do not understand why science is failing to self-correct, it will not be possible to fix it."

Yes, science needs to be fixed. Arp suggests that we need "... a more democratic science with better judgement. ... a more enlightened, more useful activity of society." It would be a wonderful, unexpected outcome, if the scientific and technological success of the cold fusion/new energy field helped lead to that more enlightened *process* of science that humanity so desperately needs.



New Cold Fusion Book Now Available

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See
excerpt
from
book on
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Introduction by David J. Nagel

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—Sir Arthur C. Clarke



LETTERS TO THE EDITOR

"E-Prize" Could Reduce Secrecy

I was disappointed by the story of excessive secrecy at Boscoli's lab (*IE*, No. 28, p. 10). This problem of paranoid secrecy has plagued the alternative energy field for a century, and there is no way to know if anything of real value has been lost because of it.

We need an E-Prize: at least ten million dollars, payable to the first inventor to publish a design leading to a reproducible, self-running energy system. This will ensure that any successful inventor will be set for life regardless of troubles with the patent system, big companies, etc.

The prize criteria must be chosen carefully so an inventor will have the incentive to publish his design immediately. If this is done right, anyone who remains secretive can be safely considered a fraud.

Mike Ingle
Camarillo, California



ACS a Historic Meeting

We want to congratulate Gene Mallove, Jed Rothwell, Akira Kawasaki, and the *Infinite Energy* staff on publishing an outstanding review of the cold fusion session at the Western Regional Meeting of the American Chemical Society (see *IE* No. 29, p. 18). In our view, this was a historic meeting. One must really look at the videos of the meeting to get an appreciation of the constructive atmosphere and helpful dialogue that accompanied the talks.

The most important results were the studies presented by Mike McKubre. They most clearly established the reality of excess heat in the deuterium-palladium system, and the quantitative correlation of the excess heat with helium product. These mass balance studies answer the major question raised by Frank Close in the early days of cold fusion: "Where is the nuclear ash?" Close was looking for the products of hot fusion and muon-catalyzed cold fusion, namely high energy neutrons, protons, tritons, and helium-3. He was not looking for low energy helium-4 unaccompanied by gamma emission. He failed to recognize that metal hydride systems can be prepared in a form that allows the nuclear attraction to come into play, forbids energetic particle emission, and provides means for radiationless de-excitation of the product nuclear state.

One could not hope for an energy ash more benign than helium-4. Now that experiments have clearly established that radiationless nuclear reactions can take place in a metal hydride, the way is open to vigorously pursue development of practical heater technology. There is some urgency in such a development program. Oil industry experts have predicted that world petroleum production will peak about the year 2010, maybe as late as 2020. If cold fusion is to contribute to easing the adjustments that must follow, it is none too early to initiate serious research in this area. We need to build on the studies described in this meeting. The road may be difficult, but the rewards are sure to be worth the effort. In addition to the economic benefits, there will be useful by-products, such as easier non-proliferation controls, further development of materials technology, a broader understanding of physics, and lower cost deep space propulsion.

Talbot A. Chubb
Arlington, Virginia

Scott R. Chubb
Burke, Virginia



CIP Engine

After finding out about the Boscoli results, I had meant to write an eloquent letter of support and encouragement. Alas, I got sidetracked by the holiday season. Let me thank you all for your diligent efforts to progress new energy technology. It must seem thankless and discouraging at times, but be sure there are thousands of us silent readers who appreciate your dedication to your chosen tasks.

I have an idea regarding the CIP engine (*IE*, No. 28, p. 61). This idea may have been tried already, or may be way off base, but I thought I would share it. I don't have time to pursue this idea now, in either thought or action, but some of your readers may.

When I was a budding young mechanical engineering undergrad, I took a class called Systems Dynamics, where dynamic mechanical systems were studied using analogous electrical circuits. I recall that things like springs, masses, "dash-pots" (viscous dampers), and other mechanical devices were substituted for resistors, capacitors, inductors, op-amps, and the like. With rationale, magic, and prodigious use of equations like Laplace transforms, one could build an electrical circuit that behaved exactly like the theoretical mechanical system.

Modeling of this type is a very efficient tool, because you can vary the parameters of the circuit cheaply and quickly, and study the resulting output with an oscilloscope or other device rather than expensive, tedious, or cumbersome mechanical testing.

In the CIP article, I noted several key points. 1) The CIP engine is a mechanical system and has been modeled on computer. 2) Inertial forces in the CIP engine were verified using strain gauge techniques on a mechanical mock-up; presumably the gauges verified the directions and magnitudes of the forces within the machine. 3) With the force vectors verified, it satisfied many that the CIP device does not follow "laws" of classical mechanics—astounding!

The combination of 1) and 2) is revealed as follows: find the analogous electrical circuit that successfully models the CIP, build it, and see what happens! Perhaps nothing, but perhaps a building block to something significant. The most challenging part of the project may be to find the electrical analog to the non-classical mechanics, or how to "start" the electrical "engine." On the other hand, there may be several great advantages to such a scheme: 1) The design is easily modeled, optimized, demonstrated, and verified. 2) The theory behind the device would revolutionize classical electromagnetism, or more. 3) It would give mechanical and electrical people, who aren't interested in or cannot grasp electrochemistry or complex theoretical physics, something to do!

It sounds simple and maybe it is—or isn't. Readers of *Infinite Energy* are no doubt well-versed in the history of science and technology, which shows us that some of the most significant advances in these fields are often remarkably simple, just as incredibly significant descriptive equations are sometimes incredibly simple ($F=ma$, $E=mc^2$...).

Time and effort will tell if this idea has merit. I suspect that others have already thought of this simple idea and are working on it. I know there are people working on solid-state "free energy" devices and maybe they have already explored this avenue.

One of my greatest hopes is that our collective efforts will come to fruition soon and that humanity will have the wisdom to apply and distribute this gift fairly and ethically.

Michael Smith
Redmond, Washington



High School Science Textbook Misinforms About Cold Fusion

Kathy Thompson screened your documentary, "Cold Fusion: Fire from Water," for her science class at Astronaut High School in Titusville, Florida in order to correct the disinformation given in the cold fusion section of the class textbook (*The Sciences: An Integrated Approach* by James Trefil and Robert M. Hazen, John Wiley & Sons). She wrote to me: "It disappoints me that scientists act on their beliefs and not hard evidence to attempt to discredit their colleagues. I guess in reality many scientists do not have the open-mindedness necessary for true investigation. In the long run, that mindset will stifle progress!"

Where the facts are at odds with established dogma, then political necessity requires that the facts, and not the dogma, be altered—a pedagogic principle that economists have long observed.

Charles Stuart
Titusville, Florida



Klass Not Objective to UFO Claims

Bruce Maccabee, in his excellent article, "Prosaic Explanations: The Failure of UFO Skepticism" (*IE*, No. 29, p. 29), notes several errors and omissions in Philip Klass' dismissal of reports of unexplained flying objects.

For many years Philip Klass has devoted his considerable intelligence and skill to absolute denial of any reality to the so-called "UFO phenomenon." I think his conduct demonstrates an intractable psychological inability to consider any evidence in this connection.

This became clear to me some years ago, when Klass and David Jacobs appeared together on CNN's "Larry King Live," following publication of Jacobs' book, *Secret Life*, in 1992. The book describes Jacobs' interviews with persons claiming to have experienced abduction by aliens, and his own conclusions based on what he learned in the process.

At some point near the end of the program, King asked Jacobs to summarize his interpretation of the significance of these interviews. Klass listened to this summation, seemingly quite impatient, and when Jacobs had finished, Klass burst out quite emotionally, almost shouting, "If these things were true, the social consequences would be intolerable!"

And so they must be, for him. But this is hardly a rational argument.

Whatever one thinks about UFO's, abduction reports, etc., it's clear that Philip

Klass, on a very personal level, simply cannot deal with this subject objectively.

Thomas E. Carey
Norcross, Georgia



Cold Fusion Fund at MIT

It is a pleasure to acknowledge the receipt of a Cold Fusion Technology, Inc. check dated December 31, 1999 in the amount of \$500 representing a grant to MIT for the Electrical Engineering and Computer Science Department.

We greatly appreciate this generous contribution and your support of Professor Peter L. Hagelstein's cold fusion research.

Bonny Kellerman
MIT, Office of the Treasurer
[Editor's Note: The amount of \$500 was part of the proceeds raised from Cold Fusion Movie Night in May 1999, which were to be used to establish a cold fusion research fund for students.—EFM]



CFT Participates in Job Shadow Day

Thank you for hosting one of our students, Bob Giguere, at Cold Fusion Technology. It coincided with Bow's first Groundhog Job Shadow Day, during which we had over 300 students meet with professionals to learn more about the world of work. As always, we appreciate your time and commitment. Thank you for sharing your knowledge in such a meaningful way! Best wishes for your continued success.

Mary Ann Gaschnig
School-to-Career Coordinator
Bow High School, Bow, New Hampshire

On behalf of the New Hampshire Department of Education, Junior Achievement of New Hampshire and the School-to-Career efforts statewide, we would like to thank you for your participation in Groundhog Job Shadow Day 2000. We are pleased to report that on February 2 New Hampshire businesses opened their doors to over 6000 students! Our common mission was to educate and inspire young people to see a connection between school and future career success.

... The incredible participation this year was a milestone that could not have been achieved without your support. Please extend our thanks to all employees that allowed students to "shadow" them and served as role models for this important event. . .

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Rachel Petz Dowd, Director
Junior Achievement of New Hampshire, Inc.



Moon and O'Rahilly Works Live On

I too am caught up in the unresolved paradoxes of classical electromagnetism. Thus, I thoroughly enjoy your updates, especially the most recent coverage of Dr. Moon's work.

I would like to know where I can find reprints of Moon/Spencer *Journal of the Franklin Institute* articles. Also, did their book *Unipolar Induction and Maxwell's Equations* ever get published?

I hope that *Infinite Energy* does indeed reprint O'Rahilly's *Electromagnetic Theory*. I was fortunate to just get hold of a copy of the 1938 version. I would be interested in having a complete copy of the 1965 version. (I believe it includes some revisions; perhaps you can correct me on this.)

Keep up the good work.

Richard Geist
Grosse Ile, Michigan
[Associate Editor Responds: Thanks for the kind words. You can usually request reprints of papers from the authors, but I think it's more polite to first exhaust the local libraries. Either a phone call or an Internet search will tell you if a particular Library has a specific journal in its stacks. As for *Unipolar Induction and Maxwell's Equations*, I don't know. I couldn't find it at Barnes & Noble, but I'll ask Dr. Spencer about it next time I see her. As for O'Rahilly, we're still hoping to bring it out again. However, the 1965 version was an unabridged reprint of the original, except it was broken into two volumes and some corrections were made to the text.—JDK]



IE Readers Do Their Homework!

Issue No. 29 of *Infinite Energy* (p. 54) featured Sinha's mathematics of low-energy nuclear reactions in crevices, voids, and channels in a solid. A descriptive study of this same phenomenon is given by Golubnichi *et al.* (P.I. Golubnichi, V.A. Kurakin, A.D. Filonenko, V.A. Tsarev, and A.A. Tsark, "A Possible Mechanism for Cold Nuclear Reactions," *Soviet Physics Doklady*, 34 (7), July 1989, 628-629) that emphasizes the high electrical fields in these micro-fissures that could produce cold fusion. Readers of *IE* would appreciate reading this non-mathematical article, because it suggests a cause for fusion and offers a method of improving the yield.

O.M. Baycura
Campbell, California



Nobel Laureate to Show Video

I've just put a link to your review of Park's *Voodoo Science* on my home page [www.tcm.phy.cam.ac.uk/~bdj10].

I'm hoping to show your "Cold Fusion: Fire from Water" video at a conference here next week.

Best wishes and good luck!

Professor Brian D. Josephson
Cambridge, UK



Extending Our Knowledge of Physics

Thank you for the copy of *Nuclear Transmutation: The Reality of Cold Fusion*. It is a great read, demonstrating what a promising field of research this is, while at the same time making clear how badly more independent research is needed. No matter what the mechanism behind the anomalous heat production turns out to be, it will most certainly extend our knowledge of physics and could very likely hold many practical applications as well.

Robert Wilcox Lewis
Somerset, Pennsylvania



New Cold Fusion Book Already Reaching Readers

I thank you for forwarding us a review copy of Charles Beaudette's book, *Excess Heat: Why Cold Fusion Research Prevailed*. As I examined this publication, I was most impressed with it and feel certain that cold fusion research and what it can provide to this world will be recognized early in this millennium.

Today's inflationary oil prices have to benefit the cause for cold fusion and wake up these sleeping giants concerning its benefits and that it will be the energy for this new century. The current situation with OPEC and the way it affects our economy has gone far enough. It is time for us to move forward and show forth our independence! Cold fusion will allow us to succeed.

This book will be reviewed and its review will be published in the fall issue of *Frontier Perspectives*. It will also be listed in the "New Books Received" segment of the coming issue of *Frontier Perspectives* due out this June. Not to mention, it will be added to the Center's library, which has a distinguished collection of manuscripts.

Again, I thank you and wish you and your colleagues success with the upcoming conference [ICCF-8].

Nancy Kolenda, Director
Temple University
Center for Frontier Sciences
Philadelphia, Pennsylvania

[Editor's Note: Charles Beaudette's *Excess Heat* is published by Oak Grove Press, but will be distributed by Infinite Energy Press. It is available now for purchase. See ad on page 5 for details.]



Voodoo Book Review

Excellent article on Park's book and attitude (IE No. 30, p. 44). It is people like him who set science back to the Dark Ages. I agree that history will pillory him as it has other small-minded bigots who simply can't understand what is going on, or have monetary/vested interests in preserving a static and inaccurate state of scientific bureaucracy.

It is up to real scientists to keep at important research so that humanity may advance.

Jon Norris
La Grande, Oregon

Errata

Ted Lach's paper, "Checkerboard Structure of the Nucleus," in Issue No. 30, had a few misprints. The equation on page 59 should be as follows:

$$r_{\text{neutron}}/r_{\text{proton}} = [2(0.96623707/1.41060761)]^{1/2}$$

On page 60, the second set of equations had one error, corrected as follows:

$$v_{\text{up quark}} = \frac{2(1.41060761 \times 10^{-26} \text{ J/T})}{2(2/3)(1.602095 \times 10^{-19} \text{ C})(0.5211 \text{ fm})}$$

In Memoriam

As this issue of *IE* went to press in late April, we learned of the untimely death of noted cold fusion theorist, Prof. Giuliano Preparata of the Dept. of Physics at the University of Milano, Italy. We understand that his death, shortly before Easter, was due to cancer of which he had been aware for only some months. Giuliano's intelligence, good humor, and dramatic style at cold fusion conferences dating back to 1990 in Salt Lake City, where I first met him, will be sorely missed. Those attending ICCF-8 in Italy next month will feel a special loss. We expect to have an obituary in a subsequent issue. —EFM



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<http://www.metaresearch.org>

The homepage of Tom Van Flandern's Meta Research. Don't be left behind as the paradigm shifts in our understanding of solar system history.

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An excellent site for keeping up with nanotechnology and the micromachines crowd. This site covers what's old and what's new, and is suitable for both newbies and old pros.

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—JDK

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AETHER



"Thus the numerical study of knots and the realization of Kelvin's dream appears to finally become a reality."

Antti J. Niemi

Anyone who understands the math of the old aether (I prefer the classic spelling to the modern "ether") theories, particularly the fluid models, knows that there is a mathematical richness available in the governing nonlinear partial differential equations to account for a host of phenomena. Even when truncated, the equations left over are adequate to deal with most problems of hydrodynamics and electrodynamics. In my opinion, the full expression of the equations may be enough to form a genuine "theory-of-everything" in physics.

Readers of this magazine are comfortable with the notion that physics got off on the wrong track somewhere. But how far back do we have to go to find the wrong turn, and, once there, how do we proceed down the right track? I think Lord Kelvin was on that right track when he was trying to model atoms as knotted vortex structures in the aether. Notice that this is a bit different from the view of the aether one finds in the popular special relativity books. There the aether is portrayed as a kind of stiff jello in which matter is suspended. But Kelvin, utilizing a perfect fluid model of the aether, hoped he could show that matter was actually *made up of* the aether, in the form of miniature vortex doughnuts. Unfortunately for Kelvin, he didn't know about subatomic structure. Also, he didn't have the mathematical tools nor the computational power available to deal with the nonlinear equations of motion one encounters with vortex knots.

I'll come back to Kelvin shortly, but I hope my comments above demonstrate that *Infinite Energy* is an aether friendly place. I bring this up because some aether papers have crossed my desk lately, and the authors seemed to feel the need to either apologize for believing in an aether, or got preemptively defensive about it. At this magazine we already know the Establishment frowns on the aether, along with everything else most of our readers find interesting. So, although I am interested in hearing about how the Establishment has mistreated our authors, comments relating to such are best left in a cover letter.

Of greater concern is that some of the aether papers reveal a notable lack of homework. If an author is unfamiliar with E.T. Whittaker's seminal work, *A History of Theories of the Aether and Electricity*, its fairly certain he's unwittingly plowing old ground. I do want to publish more aether papers in the magazine, but such papers should cover new ground. Historical papers, of course, are an exception to this rule.

I would also like to see papers that show how an aether model *better* accounts for what we know about the universe. Since the Lorentzian aether at the turn of the century was already compatible with special relativity, that topic is old news. But there are other facts that are ripe for an aetheric explanation.

It is accepted that there are four forces in the universe—electromagnetism, the strong force, the weak force, and gravity. Though Nobel Prizes have been awarded for the "unification" of the first three of these, all that is certain is that those three forces have some things in common. Clearly, they must represent different manifestations of what is going on in the aether if we're to accept that there is an aether at all.

This brings us back to Kelvin. In a viable aether theory, what constitutes a "particle"? If it is a dynamic entity of the aether itself (a vortex knot), then is it reasonable to think that the character of the flow fields both close to the particle and far from it are critically involved with the nature of charge and the strong

force? I say yes, but these questions are moot if an aether cannot support long-lasting vortex knots.

I was fortunate to come upon a paper recently by Antti J. Niemi. It's called "Hamiltonian Approach to Knotted Solitons" and it forms Chapter 15 of the book *Ideal Knots* (Stasiak, Katritch, and Kauffman, eds., World Scientific, 1998). Though Niemi never once refers to the aether, his paper is aether theory in disguise, for he investigates Kelvin's idea. He says: "In order to formulate Kelvin's conjecture in a mathematically consistent manner, we need a three dimensional Hamiltonian field theory, with knots appearing as finite energy *solitonic* [emphasis in original, ed.] solutions to the pertinent Euler-Lagrange equations of motion." In other words, can a mathematically perfect fluid allow long-lived knots to exist? What was critically important to Niemi, and should be to all aether theorists, is that the knotted solutions had to appear as genuine solutions to the equations of motion, not because some *ad hoc* knot-favorable conditions were added in.

The paper discusses the specifics of Niemi's study, and clues one into how tricky it is to work out aether theories in detail. But his study was successful and he concludes his paper with this astonishing claim: "Thus the numerical study of knots and the realization of Kelvin's dream appears to finally become a reality."

Many equate the aether in their minds with bustles and horse drawn carriages—something fit only for the gaslight era of the 1800s. This is unfortunate. One must adopt the perspective that physics discoveries in the twentieth century only put additional constraints on potential aether theories, rather than ruling them out. Niemi has shown that particles could be vortex knots. This was a big step. But can vortex knots also be protons and neutrons? If so, what happens to a neutron knot to turn it into a proton knot? And how can an electron knot have the same magnitude of charge as a proton knot, yet be so much less massive?

Looks like the right track runs out before us to the new physics horizon. □□□

A Critical Evaluation of the Pons-Fleischmann Effect: Part 1

Edmund Storms*

Abstract

Many new studies are available to make an objective evaluation of the Pons-Fleischmann effect possible. The phenomenon is conventionally known as "cold fusion," or "chemically-assisted nuclear reactions (CANR)" when the environment is emphasized, or "low-energy nuclear reactions (LENR)" if emphasis is placed on the process. A wide range of observations involving anomalous production of energy as well as nuclear products have been published. While many of the claims are still open to interpretation, the general conclusion is that an important, novel phenomenon has been discovered which deserves renewed interest.

1. Introduction

Since the claims by Professors Pons and Fleischmann (P-F)¹ were announced ten years ago, studies have been undertaken by hundreds of scientists in laboratories in at least nine countries²⁻⁴ in an attempt to verify what they then called "cold fusion." Some of the results support the idea that anomalous nuclear reactions can be made to occur in special solid materials. Many more investigations failed to show the claimed effects. Although this body of work is largely unknown to the general scientific community, nevertheless it is believed by many to show the claims to be false.

Ten years of work worldwide have produced over 2,500 published papers,⁵ many peer-reviewed, which have answered most of the objections leveled by critics. It is now possible to make a more objective evaluation of the phenomenon than was previously possible. Unfortunately, during this time the claims have been the subject of considerable distortion. The reader is asked to lay aside the emotional reaction "cold fusion" can generate and read the following arguments with an open mind.

Some readers will be put off by the clear conflict between claimed behavior and well-accepted theory of nuclear interaction presently in vogue. The author is well aware of this problem. Nevertheless, a large collection of anomalous behavior must be explained. The fact that this behavior cannot be explained by conventional theory should only serve to challenge theoreticians rather than be used as justification for rejection. In fact, many approaches to this problem are being pursued, and a few will be discussed later in the paper.

The field, which is conventionally called "cold fusion," has grown and now should be called "chemically-assisted nuclear reactions" or "low-energy nuclear reactions," depending on the emphasis one wishes to apply. The claims are supported by a wide range of anomalous behaviors involving nuclear reactions and energy production. Many methods and chemical environments have been found to produce consistent patterns of behavior. This paper will only address the claims for heat production using the electrolytic technique with palladium as the cathode and heavy water as the electrolyte, as was originally used by Pons and Fleischmann.⁶ Also evaluated will be the nuclear reaction which is proposed to be the source of this claimed anomalous energy. A broader understanding can be obtained by reading several reviews which are available.²⁻⁴

Only a small number of selected studies are examined here, to which criticisms raised by numerous skeptics are applied. An attempt is made to discover whether these objections are justified and to show why reproducibility has been so difficult. Of course, many studies have been poorly done and cannot be used to evaluate the claims either way. Such deficiencies are common in all aspects of science and, hence, can be reasonably ignored. The essential question requiring an answer is whether

the most complete studies are sufficiently free of counter explanations to allow a belief in the claims to prosper. In addition, an attempt will be made to determine how well the claims have been replicated and whether consistent patterns of behavior have been revealed. For good reason, these are the techniques science demands be followed when any new idea is evaluated.

In contrast to these conventional criteria, numerous critics have observed that "extraordinary claims require extraordinary proof." This is a very high standard, which has prematurely doomed many new ideas to the trash bin, some deserving and some not. We need to realize that potential errors can be found by clever critics in any study, no matter how well done. Hence, a perfect proof is almost impossible to obtain until considerable information has accumulated. Such

... numerous critics have observed that "extraordinary claims require extraordinary proof." This is a very high standard, which has prematurely doomed many new ideas to the trash bin, some deserving and some not. We need to realize that potential errors can be found by clever critics in any study, no matter how well done.

an accumulation is very slow and difficult if an idea is completely rejected, as has been done in this case. Consequently, at the very least, I would hope that the skeptical reader would entertain the possibility that some part of the claims deserve further study, even though all important questions have not, as yet, been answered.

This review is being published in two parts. The claims for anomalous energy are discussed in this, Part 1. The second part (to appear in Infinite Energy, No. 32) will address production of helium as the source of this energy and the proposed explanations.

2. Anomalous Energy Evaluation

2.1 Energy Measurement

For the moment we need to put aside the acknowledged mistakes made during the early work. Also we need to forget the many failed attempts at replication made by uninformed and rushed efforts early in the field's history. Although these studies had an inordinate negative effect, they were flawed by any reasonable standard, as has been argued in various papers.⁷⁻¹⁰

Some scientists require neutron emission be observed before they will accept the claim for a fusion reaction.¹¹ Significant neutron emission is not observed during anomalous energy production. The question then becomes, if the process does not involve conventional fusion, what is producing the anomalous energy? Regardless of the source, anomalous energy production is a claim worth exploring. On the other hand, critics attribute the extra energy to error in measurement or normal, overlooked chemical processes. This assertion will be addressed in the following section.

Four questions must be answered when evaluating this experimental work:

1. Was the calorimetric technique used by Pons and Fleischmann

- sufficiently stable and accurate to see the claimed extra energy?
2. Can prosaic sources of chemical energy be ruled out?
 3. Have other people replicated the claims using sufficiently stable and accurate calorimeters?
 4. Have reasons for success or failure been discovered?

Before answering the first question, a general comment about the calorimetric method is necessary. Critics have misunderstood the difference between relative and absolute calorimetry in their search for errors.¹² Although the absolute approach was used during early measurements in order to detect an immediate production of energy, this method was later found to be unnecessary, because production of energy was found to require a long wait. This delay allows a null condition to be defined, independent of absolute knowledge of power production. Any random variation in the signal is also revealed during this interval. In addition, repeated calibration during this delay can be used to demonstrate that the calorimeter is stable. Consequently, referencing the data to an initial zero value, as is done in most publications, is convenient but not necessary. Instead, power production is claimed when the signal rises above a previously steady but arbitrary background value. Seeing a signal rise out of the noise is easy and does not depend in any way on knowing the actual amount of heat being produced in the cell. The magnitude of the resulting energy is then calculated using the *change* in cell conditions and the calibration constant. All absolute values of error in temperature, voltage, current, and other conditions cancel, provided the values for these quantities have remained constant. As a result, the observed presence of excess energy is more certain than is its absolute value. If an error is to be found, it must explain how a sudden change can occur in previously stable conditions and how this change can be missed by repeated calibrations and studies of inert materials, so-called control experiments. Nevertheless, some overlooked errors are important and will be discussed.

2.1.1 Question 1: Was the calorimetric technique used by Pons and Fleischmann sufficiently stable and accurate to see the claimed extra energy?

P-F used an open cell which was not mechanically stirred. As a result, objections were raised as to the uncertain amount of unrecombined oxygen and deuterium gases leaving the cell and the uncertain temperature within the cell. In addition, their mathematical analysis of the method was complex and open to misinterpretation. Each of these complaints will be examined.

2.1.1.1 Discussion of Recombination

Electrolytic action decomposes heavy water to give D₂ and O₂, which carry away chemical energy upon leaving the cell. Should some fraction of these gases unexpectedly react to reform water within the cell, the actual amount of energy being generated in the calorimeter would be uncertain. Kreysa *et al.*¹³ immediately raised this issue because P-F did not address the problem in their original paper. Later P-F¹⁴ stated that the amount of recombination was determined by measuring the amount of water which disappeared from the cell as a function of time and applied current, and that the amount was insignificant. In spite of this statement, Jones *et al.*,¹⁵ using applied currents between 0.96 mA and 8.08 mA, suggested much later that all such anomalous heat production can be explained by uncertain recombination. Will¹⁶ did an analytical analysis of this effect and concluded that the conclusion reached by Jones *et al.* was wrong. Unfortunately, this false explanation has been widely advertised by Morrison,¹⁷ a vocal critic of the claims. These claims and counter claims are resolved in the following description of the recombination process.

At low current, the following is true and not in dispute:

1. Bubbles are small and convection currents are weak. Hence,

some bubbles can diffuse through the solution and reach the opposite electrode surface where they are available to react.

2. Electrolytic gas production is inadequate to saturate all active sites on the electrode surface. Consequently, some active sites remain and these can absorb the opposite gas, thereby allowing recombination. The presence of deposited platinum on the cathode surface increases the population of active sites¹⁸ and can enhance the process.

3. A small but fixed amount of the gases can dissolve in the solution and bathe the opposite electrode. In this case, recombination occurs after an atom has diffused through the layer of opposite gas created by electrolytic action and encountered an active site.

Therefore, a significant fraction of generated gas can recombine when the current is small. On the other hand, at larger currents additional factors operate.

4. Ions formed during electrolysis recombine to form bubbles of gas at the same active sites needed as a precursor to recombination. Consequently, a blanket of evolving gas protects the very sites needed for recombination. The amount of this gas blanket increases as current is increased.

5. Increasingly larger and more numerous bubbles create a convection current which rapidly carries many bubbles to the surface where they lose their contents to the ambient gas. The amount of this convection increases with applied current. Indeed, Jones *et al.*¹⁵ showed that simply increasing convection by bubbling nitrogen through the electrolyte had a linear effect, while reducing recombination to trivial levels.

Consequently, recombination at high current will be limited by the availability of active sites as well as by the availability of gas. As a result, the fraction of gas which can recombine on electrode surfaces will decrease as applied current is increased, regardless of how potentially active the surface may become. Indeed, Jones *et al.*¹⁵ noted that application of currents higher than those used during their published study reduced the amount of recombination. Nevertheless, they insisted on applying their experience to cold fusion studies which used higher currents.

The effect of a gas blanket was measured directly by Divisek *et al.*¹⁸ In this case, the amount of recombination was high early in the loading process, when all generated hydrogen was being absorbed by the cathode, hence not available to protect active sites or to generate convection. On the other hand, no recombination was detected after the cathode became fully loaded, when hydrogen gas was being evolved from the surface. However, this is not the interpretation given by the authors.

A study done for this paper is shown in Figure 1, where the fraction of recombination is plotted as a function of the log of applied current. Stirring the cell and use of palladium or platinum as the cathode made no difference. Also shown are the results of Jones *et al.*,¹⁵ who used a nickel cathode, and the calculations obtained by Will¹⁶ based on a model. Jones *et al.*¹⁵ found that increasing the area decreased the amount of recombination for the same applied current. This and other studies show that recombination is trivial above about 100 mA applied current. Therefore, the conclusion reached by Jones *et al.*¹⁵ applies only to cells studied at very low currents. As can be seen in Table 1, most of the studies claimed by P-F to make anomalous energy employed currents well in excess of 100 mA. In addition, excess energy has been claimed using closed cells containing a recombiner, to which this potential error does not apply.

2.1.1.2 Discussion of Temperature Errors

An isoperibolic, or ΔT , calorimeter, as used by P-F, requires the average interior fluid temperature be well-known and stable. Electrolytic action creates local heat production, which can produce

temperature gradients. Consequently, the location where temperature is measured is important. Miskelly *et al.*¹⁹ proposed that insufficient mixing occurred within the P-F calorimeter and Lewis²⁰ made this a major point in his early criticism of the P-F work.

To answer this criticism, a number of people studied this effect²¹⁻²³ and found it to be trivial. P-F answered the criticism by using five thermistors to show that the gradient was less than 0.01°C in their typical cell.²⁴ Results from a study by the author²⁵ using a similar configuration are plotted in Figure 2. Shown is the temperature gradient between the top and bottom of the fluid as a function of applied electrolytic current while energy is being supplied by an internal heater. In the absence of bubbles, a large gradient is detected. When electrolytic power is applied, thereby creating bubbles, the gradient rapidly disappears. This effect also shows the temperature error that can result when the calorimeter is calibrated using

an internal heater, which was done in many early studies.

The unanimous conclusion from this collection of experience is that bubble mixing is able to produce a sufficiently uniform temperature within the electrolyte. Therefore, the effect proposed by Lewis has not been supported by subsequent work. Unfortunately, this criticism provided a major reason for early rejection of the P-F work, a situation which continues.

Overlooked by these studies is another possible source of error. Knowledge of the amount of energy being produced in such a calorimeter requires the thermal conductivity of the wall be known and stable. Stirring changes the effective thermal conductivity of the wall because the amount of stagnate fluid next to the wall is changed. This factor is very sensitive to the amount of fluid convection next to the wall and shows no saturation as convection is increased.²⁵ This effect can be seen in

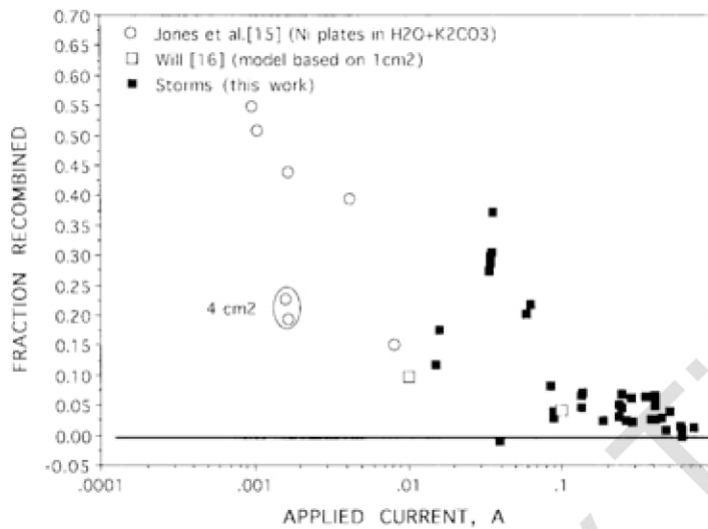


Figure 1. Effect of applied current on the fraction of evolving gas being recombined. The present work uses LiOD+D₂O with a 4 cm² cathode and Pt mesh anode. The data given by Jones *et al.*¹⁵ is converted to a fraction of recombination by dividing the measured apparent excess power by the applied power, assuming no anomalous energy is being produced. These values are based on 1 cm² and 4 cm² nickel cathodes. The calculations of Will¹⁶ are based on a cathode area of 1 cm².

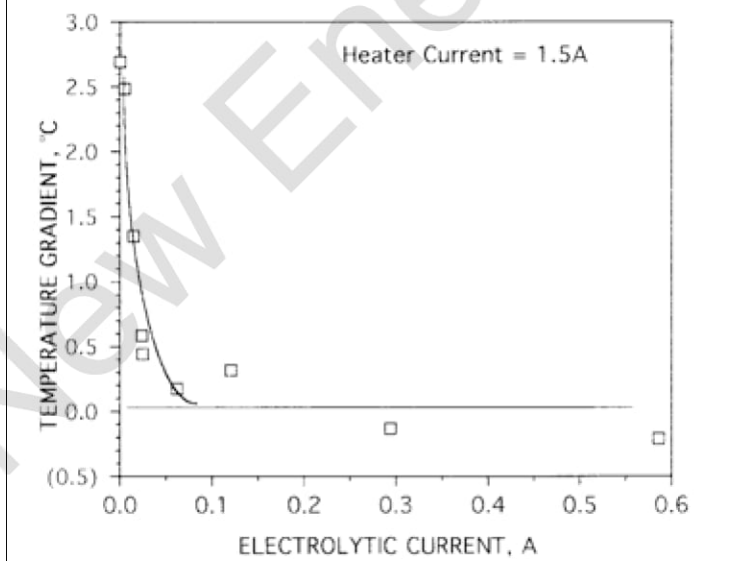


Figure 2. Reduction in gradient as electrolysis current is increased at fixed heater current.

Table 1. Excess power reported by Pons and Fleischmann using D₂O + LiOD and D₂O + Li₂SO₄ electrolyte.

Sample Type	Area cm ²	Current mA/cm ²	Excess Power W	Reference
Rod	3.1	8	0.0075	1
	3.1	64	0.079	1
	3.1	64	0.042	24
	0.4	64	0.001	24
	3.1	128	0.160	24
	0.4	128	0.005	24
	3.1	256	0.313	24
	0.4	512	0.170	24
	3.1	512	1.05	24
	0.4	1024	1.03	24
	6.2	8	0.036	1
	6.2	64	0.123	24
	6.2	64	0.006	24
	6.2	64	0.024	24
	6.2	64	0.493	1
	6.2	128	1.65	24
	0.4	128	0.028	24
	6.2	256	0.056	24
	0.4	512	0.66	24
	0.4	512	0.603	24
	0.4	1024	2.80	24
	12.4	8	0.153	1
	12.4	64	0.502	24
	12.4	64	0.263	24
	11.0	64	0.117	24
Plate	0.4	64	0.0005	24
	12.4	64	1.751	1
	12.4	128	1.05	24
	0.4	256	0.311	24
	0.4	512	2.18	24
	0.4	512	1.65	24
	128.	0.8	0.0	1
	128.	1.8	0.027	1
	128.	1.6	0.079	1
	Controls Using Light Water or Platinum			
Rod	3.1	32	-0.001	24 H ₂ O
	3.1	64	-0.001	24 H ₂ O
	3.1	128	-0.001	24 H ₂ O
	3.1	256	-0.001	24 H ₂ O
	3.1	512	-0.001	24 H ₂ O
	3.1	64	-0.000	24 Pt
	3.1	64	-0.001	24 Pt
	3.1	256	-0.001	24 Pt
	3.1	64	-0.002	24 Pt
	3.1	64	-0.003	24 Pt
	3.1	512	-0.001	24 Pt

Figure 3, which shows the relationship between effective thermal conductivity of the cell wall as a function of stirring rate. Even mechanically stirred devices will suffer an error if the stirring rate should change. Whether this effect is important depends on the frequency of calibration, the type of calibration used, and the constancy of bubble action. Therefore, the effect of this error on the various studies using the isoperibolic method is hard to judge. To the extent that it operates, one should see an apparent positive as well as negative anomalous energy. All reported data show only positive excursions. While it is possible that an experimentalist could eliminate all negative excursions as being non-real, this non-scientific approach does not appear to be common. This error would not apply to flow-type, double-layer isoperibolic-type, or Seebeck-type calorimeters, each of which has been used to demonstrate anomalous energy, as will be described in a later section. In addition, several cell designs used by P-F would be immune to the effect because the thermal barrier, *i.e.* the barrier across which heat flow is measured, was located in the gas space, above the fluid. Much of this loss was by radiation through the wall, not by conduction.

2.1.1.3 Discussion of Mathematical Approach

Three studies have evaluated the basic design of the P-F calorimeter. The first was commissioned by General Electric Company and reported by Wilson *et al.*²⁶ The two most serious problems they noted are the change in calibration constant produced by liquid level change and potential loss of heavy water with the evolving gases, thereby producing a loss of overlooked energy. The authors acknowledged that P-F avoided the effect of these errors, as well as several others, by frequent calibration. Thus, changes in cell conditions could be quickly eliminated as being the cause of anomalous energy. Although the authors claimed to find some minor mistakes, the final conclusion of this analysis is that only the magnitude of the anomalous energy can be questioned by their analysis, not its existence. Because their attempts to duplicate the claimed energy production using various types of palladium, cell designs, electrolytes, and anode metals were unsuccessful, they were not optimistic about the reality of the claims.

Next, the State of Utah requested another evaluation to be undertaken by W.N. Hansen,²⁷ a professor in the Physics Department at Utah State University. Hansen had the advantage of access to the raw data of P-F. First of all, he was able to show,

using experimental measurements, that the model used by P-F was accurate to within $\pm 2\%$ up to 90°C . Thus, the limitations proposed by Wilson *et al.*²⁶ are shown not to be important. Use of the heat-pulse calibration method is found to be completely valid. Hansen's analysis also shows that the data are consistent with excess energy being produced at normal temperatures, using the measured calibration constant, and at the boiling point, using the rate of boiloff. Examples of excess energy of 114 kJ or 470% of applied power at the boiling point and 1.5 watts for two days at normal temperatures in another case are noted. Several potential deficiencies are discussed. Cells showing the highest energy production had the largest calibration constant, and some cells were not regularly calibrated by heat-pulse. Nevertheless, Hansen concludes that sufficient evidence exists to warrant further studies.

Shortly after P-F were funded by Technova and a new laboratory was set up in France, the Japanese government created the New Hydrogen Laboratory (NHE) in Sapporo, Japan. Pons and Fleischmann worked with this laboratory in an effort to duplicate their claims. As a part of this collaboration, Saito *et al.*²⁸ evaluated the P-F calorimetric approach. They concluded, based on their study of a similar cell, that the minimum detectable power is ± 0.3 W using the model proposed by P-F, compared to a claimed uncertainty of $\pm 0.1\%$ (± 0.1 to ± 0.0002 W).²⁹ Modifications to this model allow a new proposed limit of ± 0.03 W. However, this improved approach apparently failed to reveal anomalous energy during many attempts at the NHE laboratory. This problem will be discussed in a later section.

While room for debate still exists as to the accuracy of the P-F technique, three independent evaluations conclude that P-F could detect the amount of anomalous energy they claim. Whether they actually produced anomalous energy must depend, in part, on the ability of other workers to reproduce the claims.

Before going on to examine attempts at replication, it is worth first discussing the P-F measurements in detail because they were the focus of so much criticism. Figure 4 shows a drawing of the P-F cell. In this case, electrolytic action occurs in a cell which is surrounded by a vacuum jacket that is silvered near the top. Thus, the thermal barrier is located at the bottom of the cell and at the lid. This assembly is placed in a constant-temperature water bath. Contained in the cell along with the anode (Pt) and cathode (Pd) are a glass-covered heater, a single

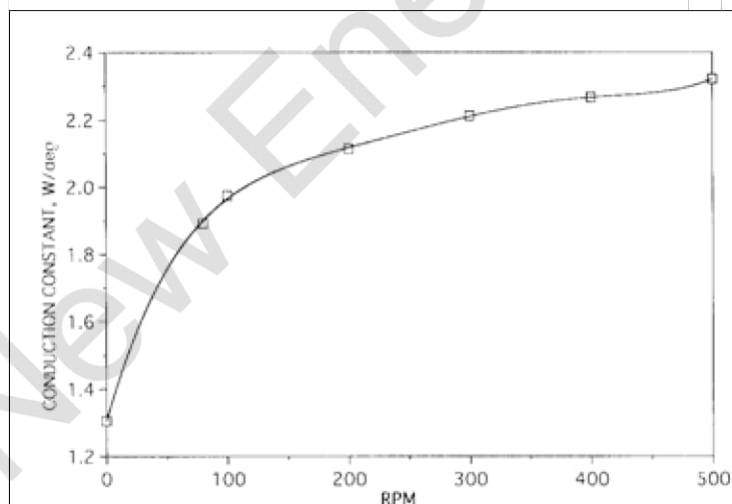


Figure 3. Effect of stirring rate on the effective thermal conductivity of a cell wall. Power is applied electrolytically to a typical cell design. Stirring is done using a Teflon™ covered magnetic bar located at the bottom of the cell.

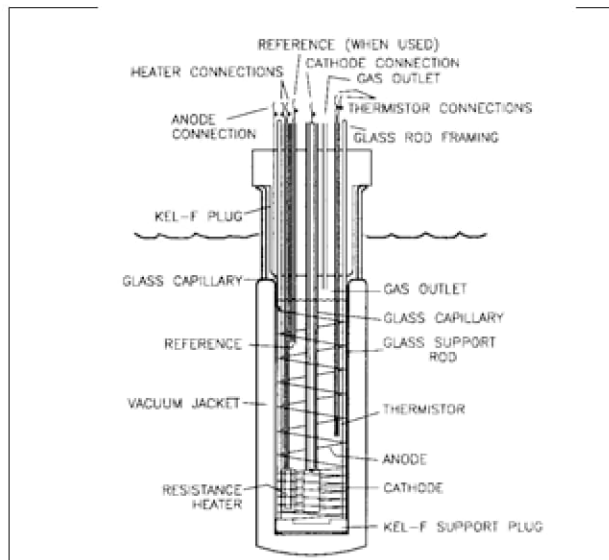


Figure 4. Drawing of P-F calorimeter. The upper section was silvered in later versions.

glass-covered thermistor, and a reference electrode. P-F answered some of the criticisms in several papers³⁰⁻³² from which the following observations can be summarized:

Currents between 25 mA and 804 mA were used with most measurements taken above 100 mA. Thus, most measurements were outside of the critical current range for internal recombination. Furthermore, they state that the amount of recombination was measured by monitoring the amount of D_2O used. Recombination was found to be no greater than 1% of applied power. Mixing was found to be adequate to eliminate temperature gradients based on experimental observation as noted above. Calibration was based on a pulse of heat applied to the submerged resistor with the resulting temperature rise and decay noted. This type of calibration allowed pulsed as well as sustained heat production to be evaluated. Heat production which caused boiling was evaluated by noting the rate at which liquid was converted to vapor. In this case, energy evolution was determined using three independent methods. These are: normal isoperibolic calorimetry (*i.e.* using a temperature difference), rate of temperature change using the measured heat capacity of the cell, and energy consumption obtained by measuring the amount of water vaporized. Each method showed anomalous energy production. Indeed, excess heat production at the boiling point has been duplicated by Mengoli *et al.*³³ and Lonchamp *et al.*³⁴

Table 1 lists excess power values published by P-F. A wide range of conditions were explored as well as a number of inert control cells. Clearly, a level of excess power was claimed which far exceeds the error proposed by the most critical evaluations of their work. In addition, they found some cells to continue producing power even after electrolytic current was turned off, so called "heat-after-death."³⁵ Recently, they have explored in more detail energy production at the boiling point. They noted an important pattern of behavior, which was that the amount of excess power increased as the amount of current was increased. The effect of increased applied current can also be seen in a partial list of other studies,³⁶ some of which used an internal recombiner catalyst, as shown in Figure 5.³⁷ The line defines the upper limit created by all reported studies as of 1991.³⁶ The scatter is now thought to be caused by variations in the properties of available palladium. More will be said about this problem later.

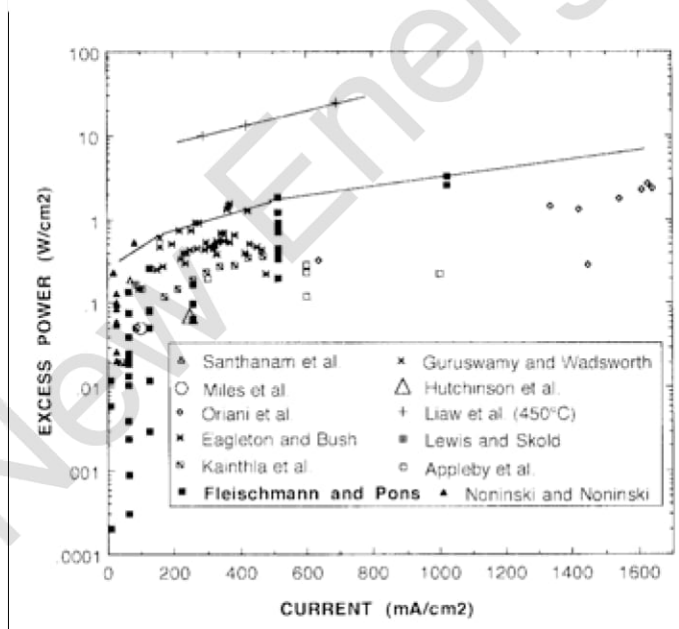


Figure 5. A comparison of power production as a function of applied current density. The line shows the upper limit of all reported values.

In contrast to the great care P-F used in describing and evaluating their calorimeter, they showed no interest in the properties of palladium. The source, treatment, and achieved D/Pd ratio are not noted. Indeed, it is impossible to know whether the listed measurements were made on a few or many samples of metal. This oversight is the main reason subsequent studies have had so much trouble duplicating the results, and it represents a major flaw in their work.

2.2 Question 2: Can prosaic sources of chemical energy be ruled out?

The hard-to-accept claim for a nuclear source is based, in part, on the belief that observed energy production exceeds any known chemical source. Therefore, the potential chemical sources must be examined. Before discussing this subject in detail, the reader should realize that a typical cell contains very few chemical components, all of which are stable with respect to each other. A chemical reaction can only be initiated by applying an electric current, a process which uses energy. Only after the water has been split into deuterium and oxygen can chemical reactions occur. This process causes several chemical reactions, including an uptake of deuterium by the palladium and slow deposition of lithium and platinum on the cathode surface. Each of these reactions involve very little energy. Strain energy accumulates in the palladium and small amounts of reaction products such as D_2O_2 can accumulate in the solution under proper conditions. These processes have the potential to store energy within the cell. Only release of stored energy can be used to explain the anomalous energy, which appears after many hours of electrolysis.

The magnitude of such processes was addressed in several papers. Kainthla *et al.*³⁸ discussed eight possible sources, including recombination, which has already been discussed above. The other sources are the energetics of PdD formation, the energetics of PdLi formation, and energy accumulation as stress. Each of these was found to be much too small to account for even the smallest reported excess energy. Handel³⁹ proposed that heat could be pumped into the cell by a temperature gradient operating between the anode and cathode lead wires, *i.e.* a Peltier effect. Two problems exist with this explanation. A significant temperature difference seldom exists between the two lead wires and the necessary difference between the Peltier coefficients of the wires must be unreasonably large to produce an observable effect. Most people use platinum for both wires, which would have a zero difference in a Peltier coefficient.

Balej and Divisek⁴⁰ compiled all necessary thermochemical values and calculated the energy involved in electrolytic formation of β -PdD. Measured energy using inert palladium is consistent with these values, as is true of all negative studies. Berlouis *et al.*⁴¹ reported a direct measurement of the heat of dissociation of $PdH_{0.9}$, formed by electrolysis, using thermal analytical techniques. The result is consistent with values obtained by using other techniques. Consequently, no anomalies have been found in the energetics of β -PdD. However, it is possible for an unknown compound to form which, when decomposed, could give off the observed energy. While such a compound has been sought, it has not been detected. In addition, such a material would have to be able to store hundreds of megajoules of energy in the few grams of palladium normally used. If this were the energy source, the material would have an energy density greater than the most powerful chemical explosive. This conclusion, if true, would be as important as a nuclear source.

Mills and Kneizys⁴² have proposed formation of the hydrino⁴³ atom as the source of energy. In this model, the electron of deu-

terium is caused by a shift to a lower level below the conventional ground-state with release of energy. The resulting shrunken atom is able to diffuse out of the cell without reacting with oxygen, hence does not accumulate or return to its original condition within the cell. This explanation is not consistent with experience, because a closed, sealed cell containing a recombiner would then experience a steady accumulation of oxygen during excess energy production, a condition which is not observed. Consequently, this effect has not been observed in typical cold fusion cells.

2.3 Question 3: Have other people replicated the claims using sufficiently stable and accurate calorimeters?

Of the more than fifty reports of excess energy, a few of which are shown in Figure 5, the work at Stanford Research International (SRI)⁴⁴ is the most complete and accurate. Initially, this work was funded by a \$6 million grant from the Electric Power Research Institute (EPRI). After this program was terminated, work was continued by IMRA, Japan at nearly the same level. Now a very small effort is being funded by the U.S. government. Over the course of this study, several designs of flow-type calorimeters were used. These share the following features:⁴⁵

1. The cells are sealed and contain a recombiner. As a result, no gas leaves the cell. Therefore, uncertainty in the amount of recombination is not an issue. Successful action by the recombiner is monitored using different methods including the change in gas pressure.
2. The cells contain a heater which maintains a constant inner temperature. Power to this heater is adjusted to compensate for any change in power production within the cell produced by electrolysis or by anomalous processes. This heater is also used to determine whether the power measurement, based on the flow rate and temperature change of the cooling fluid, is accurate. A sensitivity of better than ± 0.01 W ($\pm 0.1\%$) is claimed.
3. The electrolytic cell, its surrounding heater, and the cooling-fluid channels are all contained within a silvered, evacuated dewar in order to isolate them from the environment.
4. The whole assembly is immersed in a fluid bath which maintains a constant environment of $30 \pm 0.003^\circ\text{C}$. This bath is also the source of cooling fluid. Consequently, most studies are done at a constant temperature of 30°C .
5. A constant flow pump is used to circulate cooling fluid. Flow rate is checked periodically by weighing the fluid. Better than 98% of power produced within the cell is captured in this fluid.
6. All aspects of the measurement are under computer control, which provides continuous monitoring, and redundant RTDs are used for temperature measurement.
7. The deuterium content of the palladium cathode is determined by measuring its change in resistance.
8. Most studies involve a similar calorimeter containing an inert cathode as a reference. Both calorimeters are run electrically in series, and measurements alternate between the two systems using the same voltage and current meters.

Flow calorimetry is relatively simple and suffers from fewer errors compared to the isoperibolic method used by other people, as well as by P-F. Only four physical measurements are required. The applied power is determined by measuring applied voltage and current at the isothermal boundary, and the released power is obtained by measuring the flow rate and the temperature change of the cooling fluid. Internal temperature gradients are not important, stirring is not an issue, and uncertain recombination is not a source of potential error. Only unex-

pected changes in the measuring systems can introduce error. McKubre *et al.* have demonstrated their instruments to be stable and accurate through years of use.

On the other hand, an evaluation of this study can assume prosaic errors which might have been overlooked in spite of this experience, as several skeptics have attempted to do. Two possibilities will be examined.

1. The location of heat production within the cell might change and this change might allow a different fraction of the produced energy to be captured by the cooling water.

This assertion can be answered in three ways. First, less than 2% of the heat is missed by the cooling water. Any change in internal conditions would only impact on this small residual. Even if the total amount of residual should change, insufficient error is created to account for the observed anomalous energy. Second, the proposed effect was studied by placing a heat source at different locations. No effect was observed.⁴⁶ Third, no mechanism exists in the cell to allow the source of energy to change location. The suggested shift in recombination from the recombiner to the electrode surface would not allow a significant change to take place, as has been discussed in Section 2.1.1.1.

2. The voltage and current might be occasionally incorrectly determined because rapid fluctuations might be introduced by bubble action. These variations are proposed to be missed by the DC circuitry.

If some fraction of an AC component were to be ignored by the measuring circuit, the effect would be complex.⁴⁷ At constant current, a reduction in apparent current would cause the voltage to rise in an attempt by the power supply to restore the set current. This would cause power applied to the cell to increase, and this increase would be seen as excess energy. If, in addition, the measured voltage were lower than the actual voltage of the AC plus DC voltage component, the result could be interpreted as additional excess power. Studies by the author found a typical cell to have a maximum AC component of about 50 mV. Assuming all of this is missed by the measuring circuit and the applied current is 3A, the maximum apparent excess power would be 0.15 W. This value is small compared to power measured when 3A was applied to the quoted studies. Many studies that use small samples do not even reach this level of current. However, it is impossible to know just how this one experience might apply to all studies, because all reported data are found to become more scattered as applied power is increased. This scatter is caused by variations in the measured voltage and in temperature within the electrolyte. While these variations appear to be random, they might cause a net shift in conditions so as to be misinterpreted as excess energy. If this were the explanation, it seems unlikely that apparent anomalous heat would be reported by so many investigators using different measuring circuits, applied current, and calorimeter designs. Furthermore, this effect would not be sensitive to the nature of the palladium cathode as anomalous energy is found to be. Such an effect in the SRI studies would be observed in the reference cell as well as in the active cell. No such error was detected in this study. While these considerations do not fully answer the question, they do reduce the likelihood of this being the total explanation.

Another way to judge whether this study is observing a real phenomenon is to determine whether patterns of behavior associated with anomalous energy production are also seen by other

workers. Five significant patterns have been observed.

1. The D/Pd ratio must exceed a critical value.
2. The current must be maintained for a critical time.
3. The current density must be above a critical value.
4. Inert palladium can sometimes be activated by adding impurities to the electrolyte.
5. The effect occurs in only a small fraction of samples but more often in certain batches than in others.

Each of these claims is consistent with other studies when the necessary measurements were made. Consequently, many studies not only support the claims of P-F but the same observed patterns of behavior have also been reproduced. Let's examine each of these patterns in more detail.

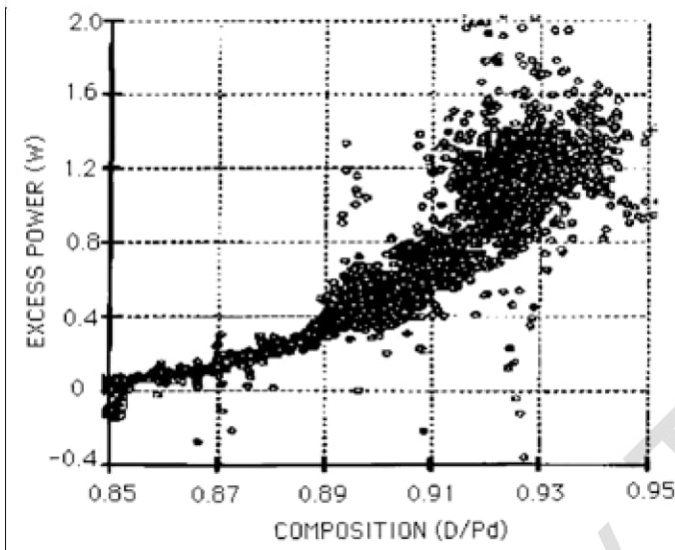


Figure 6. Typical effect of average bulk composition of a wire on the production of excess energy. Data was obtained at SRI using a flow calorimeter. Composition is based on changes in resistivity.

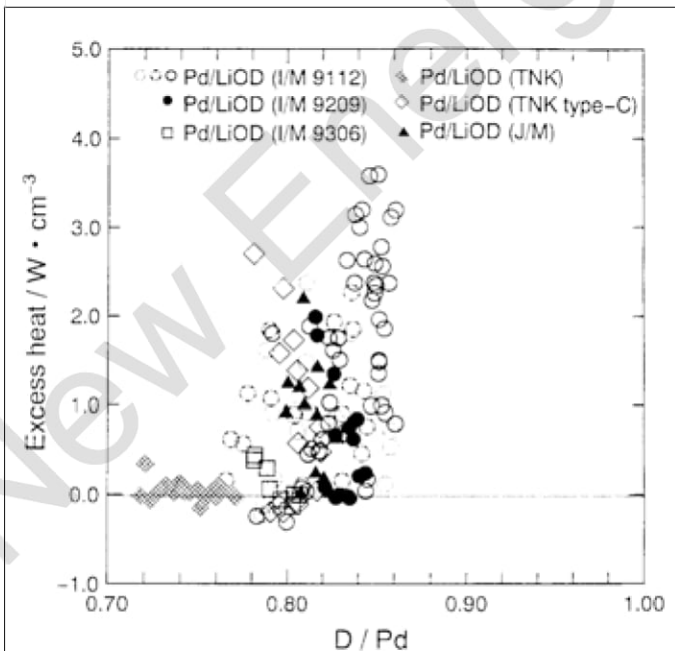


Figure 7. Effect of the average bulk composition of plates on excess power as reported by Hasegawa *et al.*⁴⁹

1. The D/Pd ratio must exceed a critical value.

Before examining the data, it is important to realize that a reported D/Pd ratio is, in every case, an average value for the entire sample. This average is sensitive to the size and shape of the sample, because the deuterium content is not uniform within the material. Edges of plates and ends of rods will experience a lower deuterium content than regions removed from these loss sites. All else being equal, wires will have a higher average composition than do plates. In addition, the method used to determine the average composition will influence the observed value. Therefore, comparison between absolute values is not possible; only trends in behavior can be compared.

Figure 6 shows the relationship between excess energy and composition for a wire sample studied at SRI. After examining 214 wire samples which gave 177,640 hours of data, McKubre *et al.*⁴⁸ report that when excess power was measured, it was detected every time when the cathode achieved a loading of $D/Pd \geq 0.95$. It was detected approximately half the time when D/Pd was between 0.90 and 0.95, and only one time when D/Pd was below 0.90.

The behavior of several plates studied by Hasegawa *et al.*⁴⁹ in Japan is shown in Figure 7. An earlier study by the same group⁵⁰ using wire can be seen in Figure 8. Other workers have shown the same effect but not in as much detail. For example, Storms⁵¹ studied a collection of plates supplied by IMRA, Japan and found excess energy production when the average composition exceeded a D/Pd of 0.82. However, some samples are occasionally found to achieve high compositions without producing excess energy. Further study²⁵ showed surface composition to be the important variable, and this quantity was not always directly related to average bulk composition. In addition, deposition of certain impurities into the surface is also thought to be required before a sample can become active.

Because evidence for anomalous behavior is only seen at isolated and scattered spots on the surface, the highly variable nature of the relationship between average composition and heat production apparently is caused by the well-known, highly variable nature of a palladium surface. Clearly some pieces of palladium contain more active regions than do others, thus the lack of consistency in the measurements.

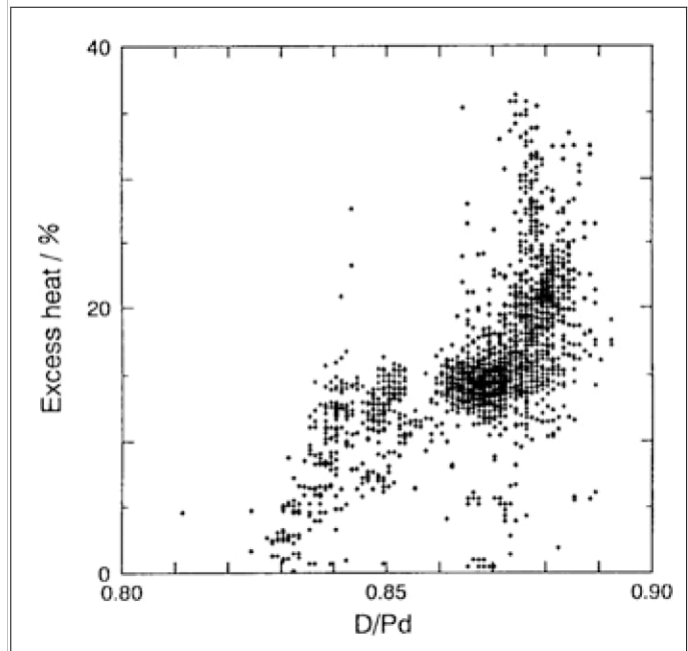


Figure 8. Effect of average bulk composition on production of excess power using palladium wire.

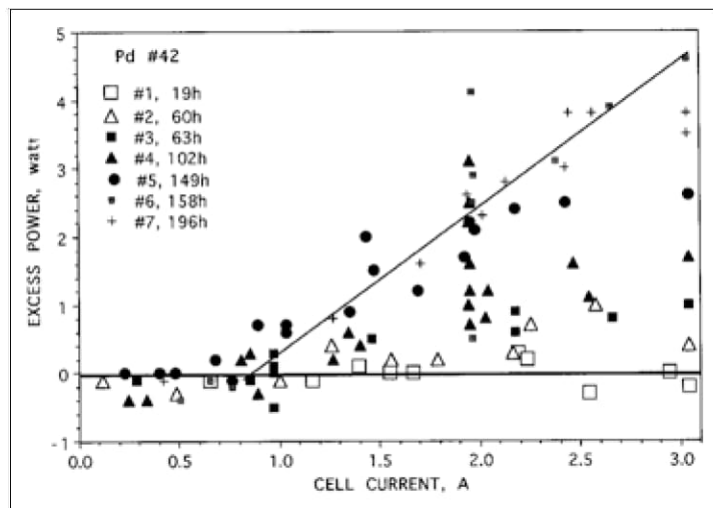


Figure 9. Excess power as a function of applied current shown after various times. The plate had an area of 4 cm².

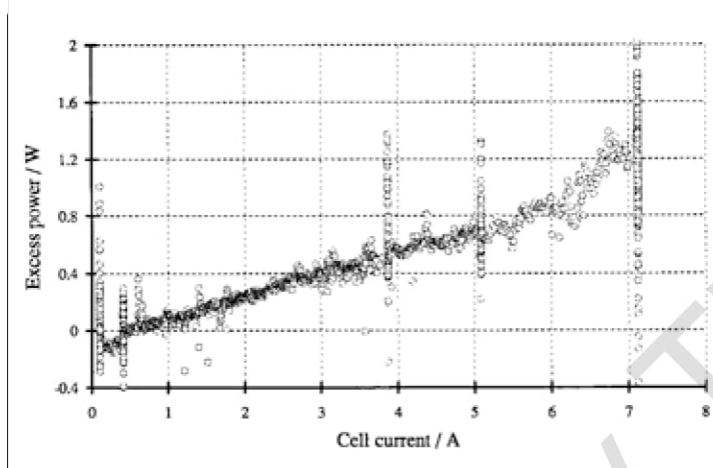


Figure 10. Typical effect of applied current on the production of excess power based on work done at SRI. The critical current varies between samples with a typical value being near 250 mA/cm².

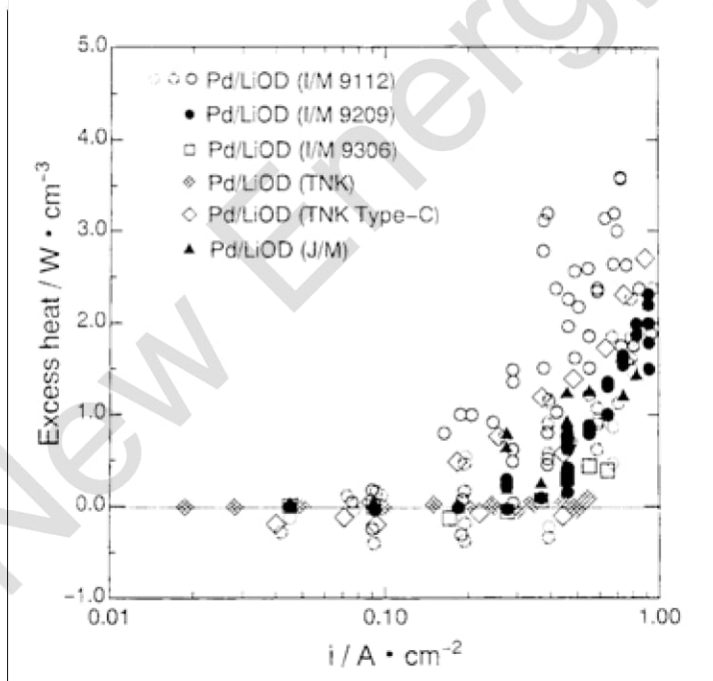


Figure 11. Effect of applied current on excess power shown by Hasegawa *et al.*⁴⁹

2. The current must be maintained for a critical time.

Every investigator reporting success using palladium has found a long delay between application of current and onset of excess energy. This delay can range from a few hundred hours to months. The delay is much longer than the time needed to form the palladium-deuterium compound. During most of the time, the composition will slowly increase as various elements are slowly deposited on the surface. These elements, which include lithium and platinum along with a steady flux of deuterium which diffuses through the surface, cause a slow change in surface properties. This change has been found necessary to produce conditions which proceed excess energy production. An example of a steady growth of excess heat production is shown in Figure 9.

Skeptics have attributed this delay to the time required for the characteristics of the calorimeter to drift, thereby indicating an apparent excess when none actually exists. This criticism has been addressed by frequent calibration. While some drift is typically found, it is not sufficient to explain the reported excess energy.

3. The current density must be above a critical value.

Figure 5 compares many studies, some of which show a clear relationship between applied current density and heat production. In addition, an example obtained by McKubre *et al.*⁴⁵ is shown in Figure 10. Similar studies by Storms⁵¹ and by Hasegawa *et al.*⁴⁹ are shown in Figures 9 and 11, respectively. Indeed, even P-F mentioned this behavior but their advice was largely ignored by those who suffered failure.

What prosaic process could cause the apparent excess energy to rise as applied current is increased?

Increased current has four major effects. It increases the AC component being measured in the DC current and voltage, increases the number of bubbles, increases the amount of energy being dissipated by the cell, and increases the chemical activity of deuterium at the cathode surface. The effect of an error in the measured current and voltage has been discussed above.

Increased bubble production will cause increased mixing. For those studies using an isoperibolic calorimeter, this can have only a minor effect, as explained above. Flow-type calorimeters would not be affected at all.

Increased production of heat could have an effect if the calibration constant were not constant but changed as the electrolyte temperature changed. Failure to recognize this nonlinear behavior could produce an error at temperature or current extremes. The author has seen this behavior after the current was applied to an isoperibolic calorimeter for a long time. Apparently, the path for heat loss can change, thereby changing the behavior of the calibration constant. However, the effect is small, causing less than a 0.5 W error. Repeated calibrations, as is done in many studies, would reveal the problem. This effect would not apply to a flow-type calorimeter such as used by McKubre *et al.*⁴⁵ and others, because such calorimeters are largely immune to where heat is being produced within the cell.

4. Inert palladium can sometimes be activated by adding certain impurities to the electrolyte.

McKubre *et al.* and others have found that excess energy cannot be produced using a Teflon™ cell unless impurities are added. Powdered glass and aluminum metal have been found to work. These materials provide elements which electroplate on the surface, thereby allowing a higher surface composition to be achieved. The exact mechanism is still unknown,

although deactivation of those active sites which permit deuterium loss by D₂ gas formation is proposed as the mechanism. Most people use a Pyrex container, which offers the needed impurities because of chemical attack by the alkaline electrolyte.

5. The effect occurs in only a small fraction of samples but more often in certain batches than in others.

Only a few organizations have the funding to allow many samples to be investigated for heat production. SRI⁵² studied 176 samples with nineteen giving excess energy. However, many of these studies were for the purpose of learning how high loading could be obtained rather than seeking excess energy. The successful samples correlated to a high degree with the average composition and many came from the same batch.

Takahashi⁵³ reports studying twenty plates, only three of which gave excess energy in the 3-5 watt level. Both Storms⁵⁴ and Kobayashi *et al.*⁵⁵ were able to replicate excess energy production using material from the same batch of palladium which produced excess power for Takahashi. Ota *et al.*⁵⁶ studied seventy-nine samples over a nine year period and found fourteen to give less than 0.25 W, five to give power between 0.25 W and 0.50 W, and only three to produce greater than 0.50 W. The rest showed no sign of excess energy. Storms studied fourteen plates and found only six that produced excess power in the 1.1-4.5 watt range. The effect correlated with the amount of cracking experienced by the samples and the average composition. Miles *et al.*⁵⁷ found that 20% of their samples gave excess energy with a high fraction coming from a few batches.⁵⁸ The results are listed in Table 2. Those samples containing boron or cesium produced a high success rate, while other samples and alloys were frequently dead. It is important to note that all samples supplied to Miles by P-F produced excess energy. In addition, a sample which was found to produce excess energy at China Lake (U.S.) also produced excess when it was studied at the NHE Laboratory in Japan.⁵⁹ Thus, once again, the effect could be duplicated when the same material was used.

This effect is attributed to the variable nature of palladium metal, especially because certain batches give a very high success rate. However, the behavior can also be explained by assuming poor stability in the calorimeters used, thus the insistence by critics that blank control cells be studied. Unfortunately, no one working in this field has had funds to support an extensive study of blank cells, although most workers have studied a few such samples. Such blanks normally use platinum in place of palladium and H₂O in place of D₂O. When blank

cells were studied, no excess energy is reported.⁶⁰ Many skeptics discount this claim by not trusting the experimenters to objectively evaluate the results, or they attribute the claimed excess power to chance variations in the measuring system.

2.4 Question 4: Have the reasons for success or failure been discovered?

The above requirements provide many avenues for failure. Success, first of all, requires palladium which is able to achieve a critical composition at the surface.⁶¹ The average bulk composition is only important because it is required to support this high surface composition. Unfortunately, most palladium forms cracks when it loads with deuterium so that the deuterium escapes faster than it can be delivered.⁵¹ Only a small fraction of available palladium does not show this behavior. Second, the heavy water must be free of normal water to prevent deuterium in palladium from being diluted by normal hydrogen.^{62,63} Because heavy water quickly picks up normal water from the air, it can easily become diluted and made inactive.

Once potentially active palladium has been acquired,⁶⁴ it must be handled correctly. This includes making sure the surface is free of finger prints and other contaminants as well as scratches. Annealing must be done in a very good vacuum to prevent formation of even a monolayer of surface impurity. The ease with which palladium can suffer surface contamination is one of the important problems which is frequently overlooked. Crystal size is also thought to be important, a property which is strongly influenced by annealing. Treatment with Aqua Regia is sometimes needed to remove unavoidable surface films. After these pretreatments, the material must be subjected to proper loading conditions. Applying only a small current for the first several days improves the chance of reaching a high composition. Only after the composition has been achieved at stable value should the current be increased into the critical range. Too fast loading or premature application of high current can produce cracking, followed by immediate loss of deuterium.^{52,65} Palladium is much more sensitive to how it is treated than most people realize.

These requirements were not known by most early workers in the field, hence success was more a matter of luck than skill. Even now, many attempts to duplicate the claims do not apply these lessons. Unless this experience is applied, a failed effort cannot be claimed as a true duplication.

3. Discussion

Clever people can always find reasons to reject any idea or measurement. Numerous unjustified rejections of new ideas can be cited.⁶⁶⁻⁶⁸ The issue is not whether data suggesting a new idea are perfect and all criticisms can be answered. Such conditions are seldom achieved even after a new idea is eventually accepted. The issue is whether the supporting data would be considered good enough to support a typical accepted idea. Would a measurement or technique be given credibility when applied to normal phenomenon? For example, calorimetry is a well-understood technique which has been applied in various forms for over a hundred years. A vast literature of chemistry and physics is based on such measurements. While measurement of power at the microwatt level is a challenge, measurement of watts, as is being done here, is not considered difficult. Professor Lee Hansen (BYU) has suggested that a calorimeter cannot be trusted unless it has demonstrated accuracy in measuring the heat from a known reaction. This is a fair request provided absolute calorimetry is used. However, as noted at the beginning of this review, relative measurements are actually being made. Stability is the only requirement, a condition which is much easier to

Table 2. Success rate experienced by Miles.

SOURCE	EXCESS ENERGY	
	Success	Total
Boron-containing samples made at NRL	7	8
J-M Pd	15	26
NRL Pd	1	6
WESGO Pd	0	6
NRL Pd-Ag	0	3
IMRA Pd-Ag	0	2
Pd-Cu	0	2
Pd-Ce	2	2
Co-deposition	2	34

J-M: Johnson and Matthey Company

NRL: Naval Research Laboratory

WESGO: A secondary supplier used early in the work.

IMRA: IMRA Japan

Co-deposition: Pd plated from solution during heat measurement.

evaluate and much less prone to hidden error. This is not to say that all claims for anomalous energy are correct or accurate. The question which must be examined is whether some studies are sufficiently correct and accurate to demonstrate the claims to be highly probable, not necessarily absolutely certain.

When such evaluations are made, the critic needs to keep in mind the potential magnitude of suggested errors. Just because an error can be imagined and justified does not mean it can explain multiwatts of apparent power production. In general, the magnitude of the effect has frequently overwhelmed any plausible errors or prosaic explanations.

The statement that the claims are not convincing is often heard. While this euphemism is actually a gentle way of saying, "I just don't believe you," one needs to ask just what is not believed and just what deserves additional study. Is it rational to reject everything just because some part does not make sense? Would it not be better to support some focused work on the subject to answer a few basic questions?

What are we to make of the consistent patterns of behavior as well as the influence of material properties and the presence of helium, a possible nuclear product as discussed in Part 2 (IE No. 32)? Is it reasonable to believe that numerous independent studies show the same patterns just because of chance? While such arguments are not a proof, they are commonly used by prudent people to evaluate all aspects of life. Indeed, this is the rationale behind requiring many duplications of a claim, a condition which has been met in this case.

When a new phenomenon is evaluated, a belief system based on probabilities needs to be adopted. An absolute rejection or acceptance is not useful. The issue is whether the likelihood of the phenomenon being real is sufficiently large so as to justify further work. In the case of the CANR claims, I suggest further study is justified.

Part 2 will explore the source of this energy and how the mechanism of its production is being explained.

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60. Some recent studies have detected excess energy after platinum has

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APS Meeting Hosts Second Cold Fusion Session by Eugene Mallove

For the second year in a row, cold fusion researchers have been granted a session to present scientific results at the primary annual meeting of the American Physical Society, an organization generally known for its hostility toward cold fusion. The latest session, dubbed "Palladium/ Deuteron," was held Monday, March 20, from 2:30-5:00 p.m. at the Minneapolis Convention Center. This was a modest step up from 1999, when the cold fusion session, with an even more obscure name, was held on late Friday afternoon as the APS meeting in Atlanta wound down.

Session "C32" was organized and chaired by Dr. Scott Chubb of the Naval Research Laboratory. Going back five years, three years before these specially designated sessions, Drs. Scott and Talbot Chubb have given individual papers within other APS meetings. (*Infinite Energy* covered last year's APS cold fusion sessions and related activities in *IE* No. 25, p. 23 and *IE* No. 26, p. 27.) A few dozen people, other than the speakers, attended the session this March, which was one of many parallel technical meetings running at the APS convention that afternoon. Only twelve minutes were allowed for each presentation, not very much time to get complex material across to the uninitiated. Still, anyone attending the session, such as some eager-looking younger physicists who stayed the full two hours, could grasp that some very big experimental claims were being made and that sophisticated theories were being presented to attempt to explain the data.

Two high-profile people who are not part of the cold fusion community attended the session. One was Institute Professor Mildred Dresselhaus of the MIT Department of Electrical Engineering and Computer Science. She is a past president of the APS, but more important, she had been one of the twenty-two participants on the infamous 1989 DOE ERAB Cold Fusion Panel, which rendered a hasty negative opinion in November 1989. Though Prof. Dresselhaus has apparently kept an open mind about cold fusion since then, thanks to continued coaching by her MIT colleagues who have continued to be involved in cold fusion, she has, to our knowledge, taken no steps to straighten things out at DOE. This day she was at least able to hear MIT Professor Hagelstein's cold fusion theory talk, as well as some of the meaty data provided by Drs. Storms and McKubre, and by Storms for Dr. Claytor of Los Alamos National Laboratory (who was unable to attend and present his paper). Prof. Dresselhaus certainly has it within her power to "do the right thing," perhaps with a letter or remark to the right people in Washington. We shall see.

The other notable in the audience was APS PR agent Dr. Robert L. Park, who sneaked in and sneaked out, staying only for twelve minutes. Unless it was mere coincidence, he had to time his arrival perfectly: he entered at the beginning of my eclectic talk, which reviewed some of the high points of cold fusion investigation, and he left just as I finished. Apparently Park was not interested in learning about anyone else's results or theories—not even to add bogus fuel to his ill-informed claims that cold fusion is "fraud." One might have thought that Park would have at least tried to give the *appearance* of a serious evaluation by staying for more of the talks. He is shameless.

The full abstracts of all the cold fusion papers may be found at: <http://www.aps.org/meet/MAR00/baps/tocC.html#SC32.001>

To review briefly the ten papers, in the order in which they were presented:

[C32.001] **Theoretical Basis for Anomalous Heat and ^4He in Deuterium-Metal Systems**—Scott Chubb, Talbot Chubb (Research Systems Inc., 5023 N. 38 St., Arlington, VA 22207) Scott Chubb made the fundamental point that cold fusion is not like hot fusion. "When D^+ and $^4\text{He}^{++}$ occupy ion band states with low concentration, however, different forms of electromagnetic coupling become possible, leading to the production of helium-4, in which EMC is violated locally but conserved globally."

[C32.002] **Cavitation Micro Accelerator and Anomalous Heat**—R.S. Stringham (First Gate Energies, 2166 Old Middlefield Way, Mountain View, CA 94043)

Since Roger Stringham could not be present, it was left to Dr. Scott Chubb to present his work. Stringham uses ultrasonic activation to produce cavitation bubbles on the surfaces of metal foils suspended in heavy water. "The steady-state calorimetry of the acoustic reactor and the acoustic power supply show the production of anomalous heat, $Q(x)$, above that of the input power. The sources of $Q(x)$ are unique nuclear events which result from jet acceleration of the deuteron high density plasma into a metal lattice." Evidence was presented for both helium-4 and helium-3 production.

[C32.003] **Low Energy Nuclear Reactions: A Millennium Status Report**—Eugene F. Mallove (New Energy Research Laboratory, PO Box 2816, Concord, NH 03302-2816)

In this eclectic talk, I denominated some of the more convincing experiments that show that helium-4, nuclear scale excess energy, tritium, and the transmutation of heavy elements can be produced near room temperature in relatively simple systems. I also gave an overview of NERL's recent activities to develop instrumentation for cold fusion demonstration cells. Mainly for Dr. Park's benefit, I flashed on the screen the cover page of my memorandum on cold fusion, which was sent to the White House (following its request) in February. He did not seem to take much notice of this. We have yet to hear it decried in his weekly "What's New" column.

[C32.004] **Calorimetric Studies at the New Hydrogen Energy Laboratory in Japan**—Melvin H. Miles (New Hydrogen Energy Laboratory, 3-5 Techno-Park 2-Chome Shimonoppo, Atsubetsu-Ku, Sapporo-004, Japan)

Dr. Miles presented highlights of positive cold fusion excess heat results that he obtained in Japan. (These—and their scandalous cover-up by NHE—were described in the last issue of *Infinite Energy*, No. 30, p. 18.) "Experiments in Fleischmann-Pons type calorimetric cells produced excess power in six out of eight experiments. These studies involved palladium alloy cathodes, co-deposition of palladium and deuterium from the solution, and electromigration using thin palladium wires."

[C32.005] **"Lochon" Catalyzed Fusion Reaction in a Solid Matrix**—K.P. Sinha (9805 Juniper Hill Road, Rockville, MD 20850)

Dr. Sinha presented the theoretical model for cold fusion which he published in *IE* No. 29, p. 54, in which lochons (local charged bosons) located around protons or deuterons and interacting with high frequency modes of the system can acquire heavy effective mass.

[C32.006] **A Requirement for Radiationless Deuteron Fusion**—Talbot Chubb, Scott Chubb (Research Systems Inc., 5023 N. 38 St., Arlington, VA 22207)

Dr. Talbot Chubb expanded on his and his nephew's theories. Referring to the Fleischmann-Pons effect in the deuterium-palladium system, he noted that Dr. McKubre says: "The evidence in my view for the appearance of an anomalous unaccounted excess heat in the deuterium-palladium system is essentially overwhelming. There is something there. It's larger by more than 1 order of magnitude, in some cases by more than 2 orders of magnitude, than the sum total of all possible chemical reactions."

[C32.007] **A Model for Fast Ion Emission from Metal Deuterides**—Peter Hagelstein (Massachusetts Institute of Technology, Research Laboratory of Electronics, Cambridge, MA 02139)

This was MIT Professor Peter Hagelstein's first cold fusion paper at an APS annual meeting. His theories of cold fusion have been much in the news over the years, and they have evolved through many iterations. Because of his steadfastness within the "bastion of skepticism" (MIT), we honor him by printing his entire abstract: "There have been numerous claims during the past ten years of the observation of a variety of anomalies in metal deuterides. One such anomaly involves the emission of fast (MeV) alpha from PdD as reported by G. Chambers and colleagues at NRL in the early 1990s. A related effect is the emission of a variety of low mass fast ions from TiD reported by groups led by G. Chambers and by F. E. Cecil (Colorado School of Mines) *et al.* (F.E. Cecil, H. Liu, D. Beddingfield and C.S. Galovich, in *Anomalous Nuclear Effects in Deuterium/Solid Systems*, AIP Conf. Proc., 228, p. 383, 1990). We have recently examined a theoretical model for this effect. We propose a second order off-resonant reaction in which fusion reactions are coupled to optical phonons, and then the optical phonon excitation is dipole-coupled to nuclear ionization. Such a model gives reaction products and energies which appear to be in agreement with experiment."

[C32.008] **In Situ Measurement of Tritium Concentration Variations During Plasma Loading of Palladium and Palladium Alloys**—T.N. Claytor, D.G. Tuggle, J. Thompson (Los Alamos National Laboratory, Mail Stop C914, Los Alamos, NM 87545)

In the absence of Dr. Claytor, Dr. Storms presented the work of this group, which continues to confirm the production of tritium in low energy nuclear reactions. (See *Infinite Energy*, No. 7, p. 39.)

[C32.009] **The Emergence of a Coherent Explanation for Anomalies Observed in D/Pd and H/Pd Systems**—Michael C.H. McKubre, Francis L. Tanzella, Paolo Tripodi (SRI International, Menlo Park, CA), Peter Hagelstein (Massachusetts Institute of Technology, Research Laboratory of Electronics, Cambridge, MA 02139)

Among the many results discussed by Dr. McKubre were absolutely stunning *multiple* curves which show the time-evolution of helium production in cells based on the catalytic fusion methods of Dr. Les Case with deuterium atmospheres. The correlation with excess heat production and the D + D reaction leading to helium-4 was remarkable.

[C32.010] **Excess Power Using Thin Layers of Palladium on Inert Substrates**—E.K. Storms (Energy K. Systems, 2140 Paseo Ponderosa, Santa Fe, NM 87501)

Excess power produced by thin layers of palladium deposited on various substrates was measured using a double calorimeter consisting of a flow-type and an isoperibolic-type combined. The effects of palladium thickness and impurity deposition were explored. The behavior was correlated with the open-circuit-voltage and the over-voltage of the cathode with respect to a reference electrode external to the D₂O-based electrolytic cell. Several potential errors associated with such calorimetry will be described. The study is directed toward producing reproducible excess energy production using the Pons-Fleischmann effect.

After the session, the full group of speakers retired to a local restaurant for respite and discussions. Later we all enjoyed the gracious hospitality of cold fusion researcher Professor Richard Oriani at his Minneapolis home. Then I caught a late night flight to Seattle, enroute to a cold fusion presentation the next day at the Climates of Change Conference in Victoria, BC (see Briefs, p. 3

9)
A good thing for my digestion, because back in Minneapolis on the next morning Dr. Park entertained a ballroom full of physicists with his session (G8) "Voodoo Science." This was a bit of physics circus and promotion "on company time" for his forth-

coming book of the same name; Park is really making a name for himself, so to speak.

We were able to learn what went on at this meeting, thanks to Dr. Melvin Miles and his wife Linda attending the session and videotaping it. There were the expected barbs flung at "What's New This Week That I Hate," plus a bit of extra firepower aimed at BlackLight Power Corporation and Dr. Randall Mills. Self-righteous statements were made about how the freedom of expression of poor physicists was allegedly being threatened by lawyers for Dr. Mills (see Briefs, p. 38). Park introduced a handful of speakers, including *Washington Post* journalist Joel Achenbach, author of *Captured by Aliens* (1999, Simon and Schuster), who professed to be in awe of all these bright physicists. In his book, Achenbach's ability to characterize cold fusion amounts to this alone (p. 129): "...the notorious 1989 announcement of 'cold fusion,' in which two scientists thought they could get more energy out of a test tube than they'd put in." He admitted that he regularly gets story ideas from Park's APS columns. Of course, we knew that already. The blind leading the blind in mutual sycophantic admiration.



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been subjected to extensive electrolysis. See <http://jedrothwell.home.mindspring.com>.

61. Storms, E.K. 1996. "How to Produce the Pons-Fleischmann Effect," *Fusion Technol.*, 29, 261.

62. Huang, H. 1990. "Effect of Light Water Additions on Excess Heat Generation of Palladium Deuterium System," *Proc. Cold Fusion Symp.*, 8th World Hydrogen Energy Conf., Honolulu, Hawaii, July 22-27, published by Hawaii Natural Energy Institute, 2540 Dole St., Holmes Hall 246, Honolulu, Hawaii 96822, p. 43.

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Strategies for Dissenting Scientists

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Abstract

Those who challenge conventional views or vested interests in science are likely to encounter difficulties. A scientific dissenter should first of all realize that science is a system of power as well as knowledge, in which interest groups play a key role and insiders have an extra advantage. Dissenters are likely to be ignored or dismissed. If they gain some recognition or outside support, they may be attacked. In the face of such obstacles, there are several strategies, including mimicking science, aiming at lower status outlets, enlisting patrons, seeking a different audience, exposing suppression of dissent, and building a social movement.

Science is normally presented to the public as an enterprise based on skepticism and openness to new ideas, in which evidence and argumentation are examined on their own merits. Trusting newcomers who present views that conflict with standard ideas may thus expect that their work will be given a prompt, fair, and incisive analysis, being accepted if it passes scrutiny and being given detailed reasons if not. When, instead, their work is ignored, ridiculed, or rejected without explanation, they assume that there has been some sort of mistake, and often begin a search to find the "right person"—someone who fits the stereotype of the open-minded scientist. This can be a long search!

Certain sorts of innovation are welcome in science, when they fall within established frameworks and do not threaten vested interests. But aside from this sort of routine innovation, science has many similarities to systems of dogma. Dissenters are not welcome. They are ignored, rejected, and sometimes attacked. To have their ideas examined fairly, it is wishful thinking to rely on the normal operation of the scientific reception system. To have a decent chance, dissenters need to develop a strategy. They need to understand the way science actually operates, to work out their goals, and then to formulate a plan to move towards those goals, taking into account likely obstacles and sources of support. The following sections cover, in turn, the dynamics of the scientific community, the problems faced by challengers, likely responses to dissenters, and strategies.

My perspective on dissent in science—which in a single article can only be outlined rather than fully justified—is based on many years' experience as a scientist and social scientist both in presenting dissenting ideas as well as studying their reception. This includes debates over supersonic transport aircraft, nuclear power, fluoridation, nuclear winter, pesticides, and the origin of AIDS, with a special focus on the treatment of dissenters.¹⁻⁷ Having given advice to many challengers, it is my perception that there is a great need to better understand the role of power in science, to be aware of the likely responses to dissenters, and to consciously examine and try out strategies.

The Dynamics of the Scientific Community

There are various ways to understand the way the scientific community operates, including as a search for truth, a puzzle-solving enterprise within paradigms,⁸ and a social enterprise in which scientists seek to enroll others, both humans and objects, to their own cause.⁹ To understand the response to dissenters, though, it is more useful to think of the scientific community in terms of "interests."¹⁰ Company owners have an interest in making a profit and scientists have an interest in publishing their papers and being recognized. "Interests" can be thought of in terms of a stake in money, power, status, privilege, or other advantages.

To talk of interests is to focus on the social organization of science. Often, when thinking about "science," people think of scientific knowledge, which is conceived of as some sort of essence above and beyond human interests. It is useful to remember that scientific knowledge is created by humans and, as a result, is inevitably shaped by human concerns.¹¹ By understanding the social dynamics of the scientific community, it is possible to gain insight into processes that influence the direction, pace, and content of scientific knowledge. The study of the scientific community then leads back to interests.

Some types of interests are corporate, government, bureaucratic, professional, career, and psychological. In each case they can exert strong pressures on the direction of research and shape the response to challengers. Note that interests influence science without the necessity of conscious bias, since people's world views are shaped by interests.

Corporations fund a large amount of research, naturally enough the sorts of research that are likely to lead to corporate advantage. A large corporation can be considered to have a "vested interest" in certain types of research and certain results, because it has used these to build a position of power which it wishes to maintain. A pharmaceutical company, for example, has a strong interest in its best-selling drugs. It will fund research into drugs that it can patent and sell, but not into non-patentable substances. It has an interest in opposing treatments that do not rely on drugs at all.¹²

Governments are much like large corporations, funding research that serves their interests. The military, a key part of the governmental apparatus, funds lots of research into weapons but very little into methods of nonviolent struggle such as strikes, boycotts, rallies, and noncooperation. The influence of governments and corporations on the direction and content of scientific research is considerable.¹³⁻¹⁵

Governments, corporations, and other large organizations are typically structured as bureaucracies, with a hierarchy and division of labor. Bureaucratic elites resist challenges to their power and privilege even when changes would benefit the organization as a whole. For example, military innovations such as accurate naval gunnery and the machine gun were resisted by military commanders for decades because they upset normal organizational arrangements. Scientific research in corporations, governments, and universities is organized bureaucratically, to some extent. Top scientific administrators have a vested interest in maintaining their power.¹⁶⁻¹⁸

Professions such as medicine and law can be understood as systems for maintaining control over an occupation, which includes controlling working conditions and entry to the field. Professions have a vested interest in this control, which sometimes is protected by laws preventing nonprofessionals from practicing.^{19,20}

Individual scientists have interests in their own careers, for example in publishing papers, gaining jobs and promotions, and winning honors. They can also develop a psychological

interest in particular theories and methods. If a challenger comes along with a simple alternative to the theory on which they have built their careers, most scientists are not likely to be receptive, since their status will be undermined and their life-long commitment apparently wasted.²¹

These different interests are often interconnected. Governments fund research by corporations and universities. Corporations fund research by medical professionals. Individual scientists build up careers in government or corporate labs.

The interests model of science is quite a contrast to the traditional model of a search for truth which is guided by norms such as skepticism, universality, and communality. The usefulness of these norms for describing science has been questioned.²² Indeed, science is possibly just as well described by "counternorms" such as emotional commitment and organized dogmatism.²¹ Using the interests model, we would expect the scientific community to respond to the most salient interests.

For example, because certain chemical companies make a lot of money selling pesticides, they heavily fund research into pesticides, do not fund much research into alternatives to pesticides, and are threatened by adverse findings about pesticides. Just as important as these direct links between interests and research are indi-

rect influences. Priorities for seemingly independent fields can be influenced by chemical company interests.

Another important interest is that of the scientific community as a whole in the status of science as a superior method of gaining knowledge. Scientific experts are given greater credibility because they are seen as having special access to truth about the world. Scientific truths are not supposed to be tainted by interests, which is why scientific knowledge is portrayed as rising above the limitations of the system that created it.

Interests are an influence on the way science proceeds, but do not determine it. There is always some scope for resistance.

Incidentally, within the social studies of science, analysis of interests has become quite unfashionable. Perhaps this is related to the field becoming more career oriented and hence less helpful to those wanting to expose vested interests!

Problems Faced by Challengers

If there are strong interests behind a particular position or theory, then the task of challengers is difficult. This difficulty is aggravated if challengers are outsiders who don't "play the game." If you are a talented scientist with a good track record, working at an elite institution, and write a conventional looking scientific paper—but with challenging ideas—there may be difficulties enough. For anyone else, it is much tougher.

If you are from a low status institution, that is a big disadvantage.²³ It is even worse to have no institution at all and to write from a home address. It is also a disadvantage to be unknown in the field, to have no prior publications, to be a female, to be too young or too old, or to be from a country with low scientific status.

Although the rhetoric about science is that it is ideas that count, not who expresses them, in practice ideas are commonly judged

by their source. Ideas are given much more credibility if they come from a respected source. Outsiders have an uphill battle.

Just as important is presenting one's ideas in the expected way. A paper, to be recognized as scientific, must conform to the standard mold. This varies from field to field, but usually means a restrained, impersonal style, suitable references to earlier work, and use of jargon appropriate to the topic, all in a concise package that is similar to other writings in the field. Anyone who writes about their own personal discovery, not mentioning prior work, and writes for a general audience, has little chance of being published in a scientific journal even if the ideas are conventional and would be publishable if in standard form. Outsiders sometimes betray their ignorance of the usual style by using ALL CAPITALS and making bold claims.

Once again, rhetoric about science might suggest that contri-

butions should be judged on their content rather than their appearance, but the reality is otherwise. Learning the standard style usually occurs during the conventional career route involving years of formal study and apprenticeship, plus working in a speciality to become familiar with prior work. Indeed, without being an active player in the field, it may be impossible to keep up, since this requires having access to the latest preprints,

attending major conferences, or knowing key people. Furthermore, without coming through conventional channels, it is often impossible to gain access to equipment needed to do the most advanced work in the field.

Arguably, one reason that science is so successful is that it is a very conservative and insular operation. By concentrating enormous resources on solving the puzzles that are on the immediate frontier, scientists are able to make steady advances and occasional breakthroughs. (Because of the role of funding and paradigms, this tends to be in areas that are useful to powerful interests.) The cost of this focus on current puzzles is a neglect of foundational questions, anomalies, and unconventional ideas.

Typical working scientists have a hard time keeping up with conventional research in their speciality. There are experiments to be done, grant applications to write, papers to be written, seminars and conferences to attend, and perhaps teaching. Research is very competitive. Delay may mean losing out to others in the field. It may mean loss of a publication, a job, a promotion, perhaps a discovery. In this context, many scientists do not want to "waste" their time looking at someone else's claim to have made a breakthrough, unless it is a top person in the field. What do they have to gain by spending time helping an outsider? Most likely, the alleged discovery will turn out to be pointless or wrong from the standard point of view. If the outsider has made a genuine discovery, that means the outsider would win rewards at the expense of those already in the field who have invested years of effort in the conventional ideas.

Responses

A person who challenges the conventional wisdom is likely first to be ignored, then dismissed, and finally, if these responses are inadequate, attacked. The first stage is being ignored.

Although the rhetoric about science is that it is ideas that count, not who expresses them, in practice ideas are commonly judged by their source. Ideas are given much more credibility if they come from a respected source. Outsiders have an uphill battle.

When an outsider sends a paper to established scientists, for example, many will not bother to reply. When an entire dissident field establishes its own publications, it may be ignored by the mainstream.

Dismissal is the most common response when seeking formal recognition in orthodox channels. A paper sent to a top journal may be rejected without being sent to referees. Editors often perform a screening function, deciding what is credible enough to warrant serious consideration. Editors can also affect the likelihood of acceptance by their selection of referees.

Sometimes, though, dissidents cannot be silenced by ignoring and rejecting them. They may develop their own constituency or gain publicity. For example, non-scientists who point out the healing power of herbs, based on their own observations, are usually ignored by medical researchers. Some researchers carry out careful studies of herbs and seek publication; they are likely to encounter difficulties or, if their work is published, be ignored by the mainstream. However, there is a thriving alternative health movement which is very receptive to any findings about the benefits of herbs. This poses a threat to corporations, governments, and scientists with a stake in the conventional approach based on synthetic drugs. At this stage, one possibility is attack.

A scientist can be attacked in various ways, including ostracism, petty harassment, excessive scrutiny, blocking of publications, denial of jobs or tenure, blocking access to research facilities, withdrawal of research grants, threats, punitive transfers, formal reprimands, demotion, spreading of rumors, de-registration, dismissal, blacklisting, and threats of any of these. There are numerous documented cases in various fields. For example, many scientists pursuing research critical of pesticides or proposing alternatives to pesticides have come under attack, having grants removed or being threatened with dismissal.^{5,24} Dentists critical of fluoridation have been threatened with de-registration.^{4,25} Government scientists critical of nuclear power have lost their staff and been transferred as a form of harassment.^{3,26} Parapsychologists have encountered difficulties in their careers.²⁷

Dr. John Coulter, a scientist at the Institute of Medical and Veterinary Science in Adelaide, South Australia, spoke out about various environmental and health issues. After he commented about hazards of pesticides in a talk, the pesticide manufacturer wrote a letter of complaint to the director of the Institute. After Coulter did a study of the mutagenic potential of a sterilizing agent used at the Institute and released his results to the workers, he was dismissed.⁷

Dr. George Waldbott, a prominent allergist and author of hundreds of scientific papers, was the leading U.S. opponent of fluoridation from the mid-1950s through the 1970s. Waldbott was visited by a German profluoridationist who misrepresented his intentions, gained access to Waldbott's files, and then wrote a critical account of Waldbott's methods. This misleading account later appeared in a dossier on opponents of fluoridation compiled by the American Dental Association and was used to undermine Waldbott wherever he appeared.²⁵

The actual cases that are publicized are the tip of the proverbial iceberg, for several reasons. Many dissenters do not make an issue of attacks, preferring to keep a low profile and continue their careers. Also, only some types of attacks are easy to document, such as reprimands and dismissals. It is very difficult to prove that failure to get a job or grant is due to discrimination.

Attacks on dissidents are never admitted as such. They are always justified as being due to inadequacies on the part of the dissident, such as low quality work or inappropriate behavior. To determine whether actions against someone are justifiable, it is

useful to use the "double standard test." Is the same action taken against everyone with the same level of performance? Or is the person who is challenging conventional wisdom harassed or reprimanded, while others with similar performance are unaffected?

Another useful test is to ask whether the response is in line with normal scientific behavior. If a scientist writes a challenging paper, it should be considered quite legitimate for someone to call or write to the scientist questioning the method or results or complaining about bias. This is a process of engagement and dialogue, and does not jeopardize the scientist's ability to continue research. Even strong language should be tolerated if it is directly to the scientist or published in a journal where there is a timely opportunity to reply. On the other hand, when a critic threatens a lawsuit or writes to the scientist's boss or institution making a complaint, this is obviously an attempt to intimidate or hinder the scientist's work or career. The "call to the boss" is very common and is an excellent indicator that a response is an attempt to suppress dissent rather than engage in dialogue.

Attacks are much the same whether they are made against scientists presenting challenging ideas, against whistleblowers who speak out about scientific fraud or corporate corruption, or against scapegoats who become targets for whatever reason. Most scientists are completely unprepared for attacks. They do not realize that science can be a ruthless power play in which the most underhanded methods may be used against those who challenge vested interests. They believe, incorrectly, that formal channels, such as grievance procedures, professional associations, and courts, provide reliable avenues for justice, when actually they are strongly weighted in favor of those with more money and power. In order to survive and thrive as a challenger, it is necessary to understand the operations of power as well as knowledge. Most of all, it is important to work out a strategy.

Strategies

Here are some ways of trying to be treated seriously in the face of hostile interests. There is no single best strategy; each has advantages and disadvantages. These options each assume some recognition that success is very unlikely by simply expecting that one's work will be treated without any bias.

Mimic orthodox science

Since mainstream scientists expect contributions to be in a certain standard format, then writing articles in this format may increase chances of success. Since submissions from institutional addresses are usually treated more seriously than those from home addresses, it may be useful to set up an institute even if it is only one person! Alternatively, it might be possible to obtain an honorary position at an established institution, such as a university. There are a few open-minded departments that may be willing to provide a haven for dissenters.

As well as the superficial appearance of being orthodox, it may also be useful to carry out research in what is said to be the orthodox manner, for example using double-blind randomized trials. Parapsychological research has followed this path and, as a result, is carried out much more "scientifically" than most orthodox science.

Sometimes this is not enough: the ideas are too threatening even when they come from the most reputable scientists from prestigious institutions and carried out using all the methods claimed to be required of proper scientific research. In this case, it can be useful to set up specialist scientific journals, with the highest standards, to give credibility to the field and provide a focal point for its workers. Parapsychological journals fulfill this function.

The more able a field is to do research and produce results that look like conventional science, the more appropriate is the strat-

egy of mimicking science: eventually the mimic will be taken for—and be—the real thing. A disadvantage is that the process of squeezing into the scientific mold may exclude some of the most exciting and provocative aspects of the field. Furthermore, if hostile vested interests are powerful the dissenters may not be accepted no matter how much they replicate the scientific model.

Aim at a lower status outlet

If it is impossible to gain acceptance in top journals and conferences, it may be possible to get a hearing in less prestigious outlets. There are thousands of journals and all sorts of conferences, of all different styles, orientations, and statuses. By picking an outlet that is less resistant to unorthodox ideas, it may be possible to gain an audience. Perhaps, from these beginnings, some people in the field will provide comment, critique, or support. This may provide the basis for building a better argument and gaining wider credibility. There are even some journals that specialize in work challenging orthodoxy, such as *Speculations in Science and Technology* and *Medical Hypotheses*.

There is no dishonor in publishing in lower status outlets. After all, most scientists go through their entire careers never publishing in the leading journals in their fields. It makes sense to publish somewhere rather than nowhere. On the other hand, it is easier for challenges to be ignored when they only appear in lower status outlets.

In the case of fluoridation, critics have long had the greatest difficulty in publishing in mainstream dental journals. Australian dental researcher Geoffrey Smith could not get past the referees for the *Australian Dental Journal* but was successful in numerous international scientific journals. Applied mathematician Dr. Mark Diesendorf had similar difficulty in publishing his critiques of fluoridation in dental journals. He made a major breakthrough with an article in *Nature*, a highly prestigious scientific journal not controlled by the dental establishment.⁴ Scientists critical of fluoridation also set up their own journal, *Fluoride*.

Enlist patrons

Is there, somewhere, an open-minded mainstream scientist who is willing to examine your ideas fairly and, if they appear to have promise, help in ensuring that they obtain proper recognition? Many challengers believe the answer must be yes and spend a lot of effort trying to find this elusive scientist. Unfortunately, most scientists are either too busy, not sufficiently intelligent or open-minded to grasp the new idea, biased by prejudice or self-interest, or have too much to lose by championing something unorthodox. The most desirable patron of a challenger is someone who is fairly senior, has excellent mainstream credentials and track record, has plenty of spare time, and is willing to take risks on your behalf. It is quite likely that there is no one who fits this specification. But sometimes there is, so it pays to inquire. By asking at a few universities for recommendations about open-minded scientists in a certain field, you may well be directed to one or two candidates.

Scientists who gain a public profile, especially those who communicate to a general audience, are obvious targets. David Suzuki, Paul Davies, and the late Carl Sagan are examples. They are likely to be totally and utterly overwhelmed by people seeking their help. It is probably better to seek someone who is known in a speciality but less known to others.

A patron can be a great help. The main problem is that lots of effort can be wasted seeking one when actually there is no one out there.

Seek a different audience

Rather than seeking to obtain credibility among orthodox

scientists, another option is to seek a different audience. This might be practitioners, those in a different field, or the general public. For example, some investigators into “alternative health” distribute leaflets to nutritionists and alternative therapists, publish articles in popular health magazines, and give talks to community groups. Some parapsychologists have obtained support from industry, which has less hostility to the paranormal than pure scientists.

The big advantage of this strategy is that it is possible to sidestep the most obvious hostile interests. If the cancer establishment is opposed to a treatment relying on a common substance, there are a number of groups that may be more receptive, including some patients and relatives, alternative therapists, and health food stores. The strategy can have many ramifications: setting up journals, newsletters, and conferences; establishing protocols and certification procedures; seeking mass media coverage.

Seeking a different audience has risks too. It may lead to an insular alternative community that cannot recognize its shortcomings due to its own interests. It may lead to associations with bizarre allies that serve to discredit what is sensible. It may make acceptance by the mainstream more difficult.

Any challenging group that develops a significant audience is a potential target for attack. Dissenters who have no following are usually ignored. Dissenters with a mass audience are a threat to the mainstream. This suggests that it is wise to develop a solid foundation of research experience and results, organizations, networks, and activist skills before gaining too wide a public profile.

Expose suppression of dissent

When attacks are made on dissenters and their work, the best response is to expose the attacks and use them to gain wider attention on the original work. Detailed documentation should be kept of all attacks, and a careful, conservative, and absolutely accurate account prepared and used to reveal the tactics of the other side. However, it is a mistake to become preoccupied by the injustice of attacks, for example by suing. Rather, the focus should always be returned to the work in question and the need for a fair evaluation.

Dissenters need to be prepared for anything. In the course of harassment, reprimands, transfers, dismissals, and other such actions, there can be unscrupulous behavior, including spreading of lies, destruction of documents, blackmail of potential supporters, and frame-ups. Most people can scarcely believe what happens to whistleblowers, and indeed can scarcely believe it when it happens to them! It is salutary to read some whistleblower stories^{6,7,28-30} and study advice from people who have dealt with whistleblower cases.³¹

Build a social movement

If vested interests are stopping the expression or acceptance of certain ideas, ultimately the only thing that will change this is a change in society, including decision making and attitudes. One way to help bring this about is through a social movement, which can be thought of as a loose alliance of individuals and groups pushing for a change in the way people do things. Conventional examples are the environmental, feminist, peace, and anti-abortion movements. Social movements normally challenge established interests; a successful movement can become a vested interest, as in the case of neo-liberalism. Some movements are not so obvious. For example, computers did not appear by themselves: there was a strong push to introduce them, which can be called a “computerization movement.”³² Science was certainly a social movement in its early years, challenging the religious establishment.

Isolated dissenters can be suppressed easily; that is the fate of

most whistleblowers. A movement, in contrast, has a better chance of gaining a hearing since it combines the skills and resources of many like-minded people who are committed to a cause and who can support each other. It is worthwhile for dissenters to contact activist organizations that are related to their area. Many activists have great skills in analyzing local power structures, mobilizing support, and building campaigns.^{33,34} Building a social movement is not a quick road to success but in the long run it may offer the best prospect for challenging vested interests.

The social system of science has forged enormously strong links to governments and corporations and as well has developed vested interests in education systems, career structures, and organizational arrangements. Indeed, science itself can be seen as a social problem.³⁵ Many aspects of the practice of science do not live up to the high ideals of "science" as a dispassionate search for truth. If there is any hope of reform, dissenters have a crucial role to play. To be effective, they need to understand that science is a system of power as well as knowledge, and that consequently they need to be prepared for a power struggle as well as a struggle over ideas.

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BBC "The Works" Program Features Segment on Cold Fusion

*Partial transcript from January 13, 2000,
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"The Works" program broadcast on BBC World Service.*

Dee Palmer: Limitless, cheap, pollution-free energy. Many technologies have promised that, but none has yet delivered. For instance, physicists have been working for decades on nuclear fusion, a process that occurs in the Sun, where hydrogen atoms fuse together to form helium and release heat. But, earthbound attempts to recreate the process in large, expensive reactors have had little success. Then, in 1989, two scientists at the University of Utah in the United States announced that they'd engineered the fusion of hydrogen atoms on their lab bench at ordinary temperatures. The world went cold fusion mad. Limitless, cheap, pollution-free energy seemed a real possibility. But other researchers couldn't recreate the results and the Utah scientists were discredited. However, a few people have carried on the work and are beginning to show that there may be something in cold fusion after all, as Rory Johnston now reports from California.

Rory Johnston: When the American Chemical Society met recently near Los Angeles, a session was arranged to discuss cold fusion. There were protests that the Society should not support craziness. The resulting publicity attracted so many delegates that the cold fusion meeting had to move to a bigger room. The organizer was Dr. Melvin Miles, a research chemist at the U.S. Navy's Air Warfare Center in California. He explained to me that cold fusion experiments involve the rare metal palladium, deuterium (or heavy hydrogen—every atom has an extra neutron), and heavy water, which is water made with heavy hydrogen.

Melvin Miles: The cell actually is fairly simple. It's a palladium rod—a small piece of palladium metal—that's used as a cathode in electrochemistry and the platinum cage around it that completes the circuit and you pass electricity through it in electrolyzed heavy water, which forces deuterium, an isotope of hydrogen, into the palladium. Palladium is well-known for its ability to take up a considerable amount of hydrogen or deuterium. After several weeks of this—it takes some time for this to get going—the calorimetric measurement, the measurement of heat, you get more heat coming out than you put in.

Rory Johnston: And where do you think this heat is coming from?

Melvin Miles: I'm pretty confident that the excess heat comes from a fusion reaction involving two deuterium atoms, or ions, inside the palladium fusing together by some unknown process to form helium-4 as a product. That produces 23.8 MeV of energy.

Rory Johnston: At California Polytechnic in Pomona, physics professor Robert Bush showed me around his lab, where he is doing similar work but using nickel and ordinary light water.

Robert Bush: What we're looking at is an electrolytic cell set-up and you have a small electrolytic cell sitting in a bath contained in a copper vessel and essentially we've got two thermocouples in the cell that tell us temperatures at two different parts of the cell. The cell has a cathode, which consists of nickel fibrex, and then

you've got a wire along the central axis, which forms the anode. When we're running this, the current is flowing through the electrolyte from the anode to the cathode and it's the cathode where the protons go into the surface of the nickel fibrex and where we're getting these what we think are nuclear reactions. We're certainly getting more energy out than what we're putting in.

Rory Johnston: The problem for many scientists is that they think fusion could only take place in the extreme heat of a giant reactor, like a tokamak, or inside the Sun. Dr. Bush thinks he sees an explanation.

Robert Bush: In the tokamak, which is the paradigm for nuclear physics now, you've got a sort of a magnetic bottle which holds a very tenuous plasma in for short periods of time and the vast number of reactions there are strictly two-body reactions. And, so, essentially, one of the rare reactions would be two deuterons colliding head on to give you a helium-4 nucleus, plus 23.8 MeV gamma ray. In the solid state, though, in the palladium lattice, say in the nickel lattice, a totally different situation prevails there. And the reason it's different is that basically you've got the ordering of the lattice and the chance for many-bodied reactions goes way up. Very clearly, we have new possibilities for new physics here and this may be the physics that will account for the nuclear reaction.

Rory Johnston: Dr. Mike McKubre is working with palladium at SRI International near San Francisco, one of the world's leading research institutes. The problem with making energy from cold fusion at the moment, he says, is that 30% excess heat is not enough.

Mike McKubre: You could never make a practical, economically useful device if your power multiplier is only 1.3. You need a factor of 3 before you can put a device on the market and sell it for profit. So, we have to increase the effect, in scientific terms, intensively rather than extensively. We have to make the effect a larger fraction of the input. Then we'll scale it up and make the devices much bigger and more practical.

Rory Johnston: Doing this will take a lot of work, he points out, and that needs money.

Mike McKubre: We cannot secure that level of funding unless we have evidence which can't be compromised or disdained by a nuclear physicist who simply hasn't bothered to look at the evidence. So, we need something that the average politician, the average banker, or the average person can understand and understand the significance of, and I think we're pretty close.

Rory Johnston: There's also work going on in Italy and Japan. Some of the most remarkable results are coming from another U.S. Navy lab in San Diego, where Stanislaw Szpak and Pamela Boss are detecting the production of tritium. Tritium is doubly heavy hydrogen, extremely rare in nature and radioactive as well, so it's unmistakable. Pamela Boss thinks this could convince even the most hardened skeptics.

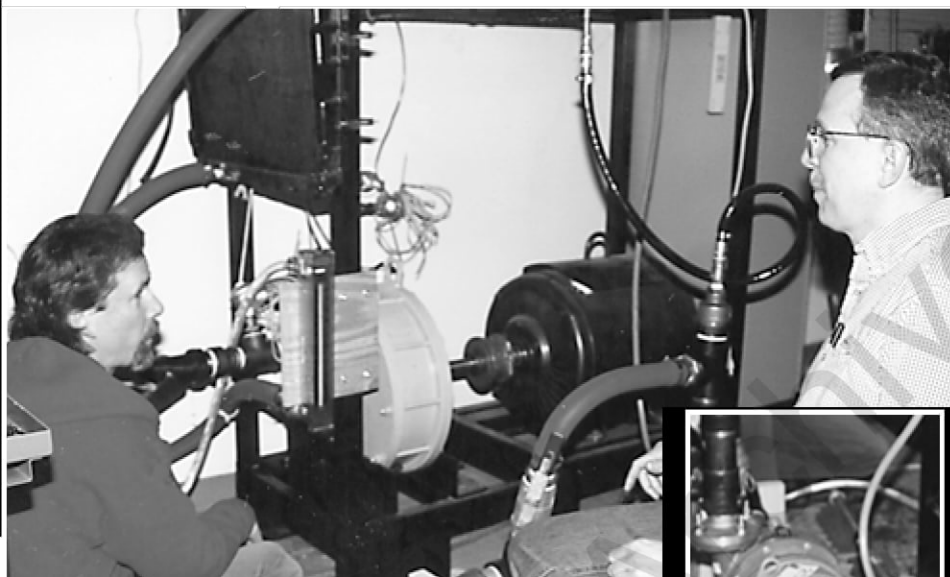
Pamela Boss: If people get open-minded and don't have preconceived ideas and just look at the evidence and realize that, yeah, there's something strange going on here and that you should look at it in a different point of view. Don't look at it from traditional physics. Just look at the data and then just say, "Okay, this is what the data is showing us. We accept it as true. Now let's try and explain it."

Dee Palmer: Pamela Boss ending Rory Johnston's report.

New Energy Research Laboratory

Device and Process Testing Update

Ed Wall



Ken Rauhen (L) and Ed Wall discuss progress on the Hydrosonic Pump.

Photo by Barbara DelloRusso



Hydrosonic Pump

The heart of the Hydrosonic Pump is a spinning aluminum cylinder immersed in water with indentations drilled into its rim, which cause cavitation of the water due to the extreme shear forces on the water. Anomalous heat production has been claimed for this device.

The Hydrosonic Pump unit testing has proved to be more challenging than expected. Providing power proved to be time-consuming. We still have not tried turning on the Hydrosonic Pump motor at our new laboratory, but we expect no problems. This will have to be done at night, as required by the power company, just in case the motor does cause a problem for them.

An ideal testing situation would be one in which the test could be run for an indefinite period without affecting its environment temperature, where the inlet water temperature and flow rate would be constant. This might be realizable if we had a pair of huge water tanks in a temperature regulated environment, but that is impractical. Besides, these tanks would have the problem of changing pressures as the water level changed.

Ten gallons/minute is a good target range for testing, according to Kelly Hudson of Hydrodynamics, Inc.¹⁻⁷ The water to our building has a flow rate that is closer to one gallon/minute, so the idea of a closed system utilizing an auxiliary pump and radiator made a lot of sense (see diagram). It would allow steady-state conditions to be attained for an extended period if the heat could be removed from the system at a steady rate. The room in which we test has a high ceiling, and we can open two garage doors, so the air around the device could probably be maintained at a pretty constant tem-

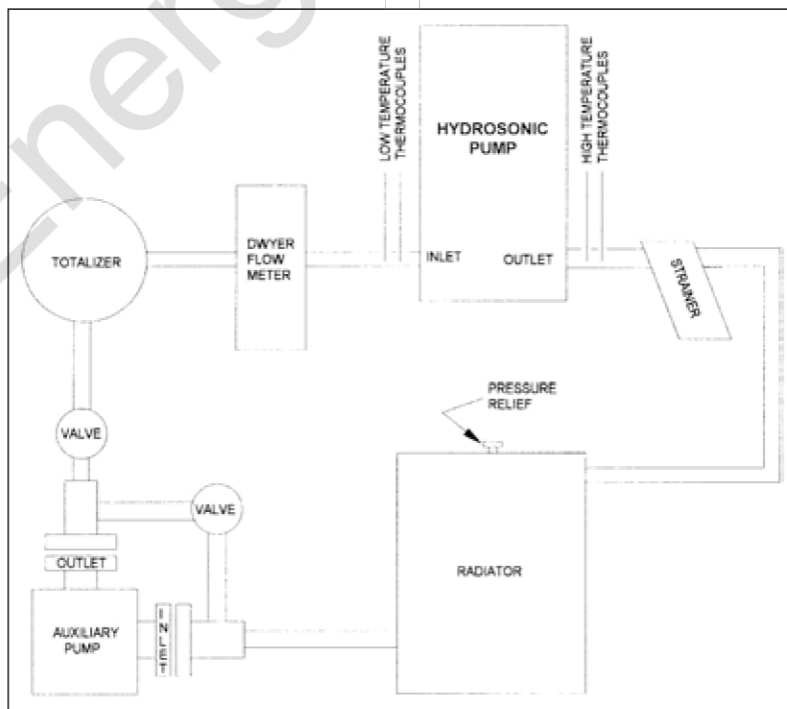
perature for many hours.

A fantastically good deal on an unused large surplus brass centrifugal pump, worth about \$2000, came along and was too hard to resist at \$165, but had a flow rate closer to 80 gallons/minute. This seemed like a minor problem, because we could divert most of the output flow back to the input and use a valve to control the flow rate delivered to the Hydrosonic Pump.

We have a fairly precise Dwyer water flow meter that is rated at 2% of full scale (10 gallons/minute) accuracy, along with a totalizing water meter (total quantity measured) that has good resolution. After a rather interesting plumbing adventure, we were able to turn on our circulating pump and have it pump at different rates through the water meters. A 9% difference was found between the two meters, so another reference method is a must. We intend to divert the flow into a barrel while drawing water from another barrel and measure the time it takes to pump a known volume.

This change of configuration may affect the flow rate, but this is not a big problem, because the two meters' flow rates will be recorded during barrel volume and timing measurement. By running the pump at different flow rates, a calibration of the flow meters can be performed.

For a while, it looked like we would be generating Hydrosonic data soon, but an unexpected complication happened. The inlet side of the pump is at a lower pressure than atmospheric. The pump is pumping so vigorously that it heats up the water rapidly. There are only about four gallons of water circulating. That is not a big problem,



rapidly. There are only about four gallons of water circulating. That is not a big problem, because we can blow air through the radiator and keep it cool. A likely problem will occur when we turn on the Hydrosonic Pump and the water gets hot. To see what the auxiliary pump will do when the water gets hot, we let the auxiliary pump run for a while and heat the water. As water heats, its vapor pressure increases, so it can boil at the pump inlet, where the pressure is the lowest in the circuit.

A rapid thumping sound developed at around 55°C and gradually slowed in frequency while increasing in intensity as the temperature rose. When the water temperature approached 60°C, it sounded like a water hammer, then suddenly quiet. The water flow stopped completely as well. This was a big surprise, and it soon dawned on us that it would be a much bigger surprise if the Hydrosonic Pump had been allowed to heat the water to this range. The sudden cessation of flow would mean that the water that was in the Hydrosonic Pump would suddenly boil, producing superheated steam, which reaches high pressure, and would probably destroy our radiator and plumbing, and could scald an unwary witness. There is a pressure relief in the form of a radiator cap, but a surge of pressure like one caused by a loss of water flow would be far too much for that.

So, the plumbing was reworked with a larger diameter pipe and hose in order to maintain a higher pressure at the pump inlet. This seems to have had its desired effect, as the flow velocity is much reduced at the pump inlet, and we were able to run the temperature up to above 75°C when the seals failed on one flow meter and the other one just stopped working. The readings on the meters diverged at elevated temperatures, so calibration of both must be performed at a range of temperatures.

The best way to proceed now is to provide sufficient air flow through the radiator to keep the temperature of the water flowing through the flow meters at a low level, then calibrate flow measurement in that temperature range. We would like to try a range of operating water temperatures, and perhaps pressurize the system a little (with an inert gas) to see if any affect on energy efficiency is apparent.

Dash Cell

We are continuing development of the Dash cell experiment apparatus⁸ and can report that cutting Pyrex is something that should be approached carefully. The recombiner is held in a chamber made from Pyrex and Teflon. The recombiner itself is the same catalyst used in the Case cell, manufactured by United Catalysts, Inc., which Dr. Dash finds to work very well.

A Thermometrics Seebeck Envelope Calorimeter (SEC) was ordered, and is expected to arrive soon. We actually began construction of our own SEC, but the potting compound, which was advertised specifically for thermocouples, etched out the solder from our device. This was a serious aggravation, as our SEC contained over 1500 thermocouples, and its performance was quite good (sensitivity: 0.045 V/W; noise floor of 1 mV without thermostatic reference; very low heat source position sensitivity). Another SEC is under construction with a different type of potting compound, epoxy. A nichrome wire heater with a precise resistance was made for use with a wall-mounted power supply in a feedback controlled water heater for the SEC thermostatic reference, but the same potting compound destroyed that as well.

The 'condensation' that was reported in IE No. 29 on the inside of Dash's SEC turned out to be leakage from the water jacket. It has been repaired and their group is back to producing data.

Case Cell Retrospective

In the process of testing a proprietary device, we discovered that the valve used in the Case catalytic fusion cell work was not that good for vacuum work; it may have admitted considerable oxygen. It is hard to tell. Better valves are an

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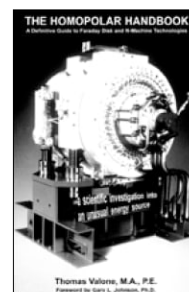


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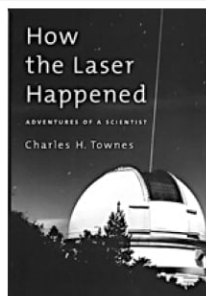
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Book Reviews

How the Laser Happened: Adventures of a Scientist

by Charles H. Townes
ISBN 0-19-512268-2
\$29.95 Hardcover, 200 pp.
Oxford Univ. Press, 1999



Review by Jed Rothwell

In this old-fashioned short autobiography, Nobel laureate Charles H. Townes describes the high points of his career and his philosophy, but not much about his personal life outside the laboratory. His most productive years spanned the golden age of twentieth century applied physics, from the 1940s to the 1960s, when everything fell in place and loads of money was available without strings attached.

The writing style is a little wooden in places. Some readers will be lost in the technical details in some chapters, or the legal details in the chapter about laser

patent wars, but overall the book is captivating. It is like visiting an old-fashioned, self-deprecating, slightly naïve professor who takes things literally, loves his work, and has a corny sense of humor. He tells of many interesting and amusing “adventures,” including day-to-day incidents in the laboratory which are as much fun to read about as the great breakthroughs. In his plain-spoken fashion he makes the old verities vivid and refreshing: the primacy of experiment over theory, the importance of teamwork, sharing ideas, keeping an open mind, and enjoying your work.

People never seem to learn that your logic may be right but your facts and assumptions may be wrong. You can never depend on logic. . . In science, there is usually no cold, objective inevitability to discovery or the accumulation of knowledge, no overarching logic that controls or determines events. There may be broad unavailability to some discoveries, such as the maser, but not to their timing or exact sequences of progress. One has ideas, does experiments, meets people, seeks advice, calls old friends, runs into unexpected remarks, meets new people with new ideas, and in the process finds a career of shifts and often serendipitous meanders that may be rewarding and rich, but is seldom marked by guideposts glimpsed very far in advance. (p. 75)

Townes works by induction, starting from theory. He does not perform an experiment until he feels he has mastered the physics. The kinds of discoveries he made and his method of working are as different from cold fusion as science can be, since cold fusion is based on experiments alone, albeit with emerging theoretical understanding. Yet in practice, the “nuts-and-bolts” challenges he describes sound a lot like those in a cold fusion electrochemical laboratory:

... The problem was that our cavities were a bit too lossy (too low in Q) to allow a resonant wave to build up as it bounced back and forth. We solved this problem little by little.

The idea all along was that, as we sent the molecular beam from one end of the cavity to the other, amplification would occur in microwaves reflecting back and forth across the cavity, more or less at right angles to the direction of flow in the molecular beam. Originally, we felt the whole cavity should be as enclosed as possible, but this was inconsistent with the job of getting molecules in and out. . . We kept try-

ing different metal rings fitted into the necessary holes in the ends of the cavity. They could perhaps keep microwaves from escaping too fast while letting molecules pass. The solution, when it came, was simple. One day, Jim Gordon opened the ends almost completely; that was what put it over the top. Without a ring in each end, we could get plenty of molecules through the cavity. Our worry over too much radiation leaking from the ends was unnecessary. Apparently, without the rings, the pattern of radiation in the cavity became simpler and more efficiently confined. . . (p. 66)

Townes and his research team were able to make rapid progress because they openly discussed the problems and worked as a team. Unfortunately, few groups of people are working on cold fusion with this kind of spirit and openness. Most researchers work alone and restrict the sharing of data or battling around of ideas. At places like Hokkaido University and Portland State University, where cooperative teams are at work, encouraging progress is being made.

One of the discouraging themes of the book is the closed-minded rigidity of establishment scientists. Townes does not dwell on this, and he doesn't seem to be bothered by it much, yet he keeps returning to the subject. In 1945 he proposed building a radio telescope. The head of the Mount Wilson-Palomar Observatory discouraged him, saying, “I don't think radio waves are ever going to tell us anything about astronomy. . . The waves are too long. . . They're not directional, they can't really tell us anything.” (p. 42) Townes comments: “People in well-developed fields tend to be conservative, particularly with regard to ideas from outsiders. As experts, they have a feeling that they understand the field well and often do not much care for interlopers.” (p. 42) Yes, we know that, but it is good to hear it from a Nobel laureate establishment insider.

He describes the stubborn resistance to his groundbreaking work on masers (which later led to the better-known laser). The first working maser took three years to build, 1951-1954. It was constructed by Townes and three graduate students: Gordon, Zeiger, and Dousmanis. Many people thought the project was a mistake:

One day after we had been at it for about two years, Rabi and Kusch, the former and current chairmen of the department—both of them Nobel laureates for work with atomic and molecular beams, and both with a lot of weight behind their opinions—came into my office and sat down. They were worried. Their research depended on support from the same source as did mine. “Look,” they said, “you should stop the work you are doing. It isn't going to work. You know it's not going to work. We know it's not going to work. You're wasting money. Just stop!”

The problem was that I was still an outsider to the field of molecular beams, as they saw it. . . I simply told them that I thought it had a reasonable chance and that I would continue. I was then indeed thankful that I had come to Columbia with tenure. (p. 65)

Six months after they built the first maser, they completed another one and operated the two together in tests which proved the functionality of the devices and purity (regularity) of the signal. Skeptical opposition continued for a few years:

Before—and even after—the maser worked, our description of its performance met with disbelief from highly respected physicists, even though no new physical principles were really involved. Their objections went much deeper than those that had led Rabi and Kusch to try to kill the project in its cradle. . .

Llewelyn H. Thomas, a noted Columbia theorist, told me that the maser flatly could not, due to basic physics principles, provide a pure frequency with the performance I predicted. So certain was he that he more or less refused to listen to my explanations. After it did work, he just stopped talking to me. . .

I visited Denmark and saw Niels Bohr. . . I described the maser and its performance. "But that is not possible," he exclaimed. I assured him it was. Similarly, at a cocktail party in Princeton, New Jersey, the Hungarian mathematician John von Neumann asked what I was working on. After I told him about the maser and the purity of its frequency, he declared, "That can't be right!" But it was, I replied, and told him it was already demonstrated. (p. 69)

When cold fusion research began in the 1920s, there was no opposition, and there is no intrinsic reason why there should be unusual hostility today, beyond the level Townes overcame. I do not buy the idea that oil companies, the DOE, or other vested energy interests are fighting cold fusion to prevent competition. A breakthrough in wind power or solar cells could profoundly disrupt the energy market. It would not put the oil companies out of business, but it might take away a third or half their business, the way foreign car imports hurt U.S. auto manufacturing in the 1970s and microcomputer sales hurt mainframe computers in the 1980s. Despite this threat, the oil companies, the DOE, and hot fusion scientists do not attack solar power or wind power. By 1999, wind power accounted for over 10,000 MW, equivalent to the amount of fission nuclear power installed worldwide by 1968, and it was growing rapidly. The oil companies must have considered nuclear fission a serious rival in 1968.

Mainstream scientists tried to prevent Townes from making the maser, because they thought it was theoretically impossible and a waste of time. This is why most of them opposed cold fusion at first. Unfortunately, once emotions were aroused, opposition fed upon itself. People went out on a limb to defend their initial conclusions. There are always more new ideas that we can afford to implement, so all new ideas face competition, and opposition. Many scientific breakthroughs are delayed by extraneous cultural factors or accidents of history. Hormonal oral contraception was proposed in the late nineteenth century. By the 1920s the principles were well-understood, but research did not begin until the 1950s, and there is no telling how much longer it would have been delayed if it were not for the indomitable Margaret Sanger. The pill was developed in 1956 and approved for use by the FDA in 1960. Other countries followed slowly. The Japanese government finally legalized oral contraceptives in 1999. Emotional reactions to birth control are understandable, but in other cases opposition seems to be baseless, or downright loony. Most Europeans accepted pasteurization soon after 1860, but the dairy interests prevented effective pasteurization of milk in New York City until World War I, because they did not want to spend an extra penny per bottle for the fuel and equipment to warm the milk. It never occurred to them that killing off thousands of infants per year reduced customer confidence and hurt their sales. In 1965, the automobile companies should have embraced Ralph Nader and enthusiastically installed seat belts and other safety features, but they fought him instead. Nowadays they advertise safety features. In the 1950s, for some obscure reason, right-wing organizations in the U.S. became convinced that the International Communist Conspiracy was behind public health measures to fluoridate water. I believe most of the opposition to cold fusion is an accident of

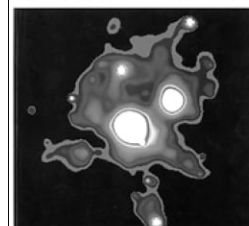
Seeing Red: Redshifts, Cosmology, and Academic Science

by Halton Arp
ISBN 0-9683689-0-5
Illustrated, 12 color plates
\$25.00 Softcover, 314 pp.
Apeiron, 1998

Quasars, Redshifts, and Controversies

by Halton Arp
ISBN 0-5213631-4-4
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\$38.95 Hardcover, 198 pp.
Cambridge Univ. Press, 1987

SEEING RED



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Review by Eugene Mallove

Though we are familiar with books that challenge reigning scientific paradigms, these by astronomer Halton Arp, written about a decade apart, relate such an astonishing story of data denial and obstruction of frontier science that had we not experienced the "cold fusion war" firsthand, we might not believe them. Surely mainstream astronomers couldn't be *that* blind or *that* stupid. Sadly, some of them appear to be just that. Just who are these astronomers and astrophysicists, supposedly among the most

open-minded of scientists, who reject data that could radically alter our most fundamental world view?

Dr. Arp's books must be ranked near the top of a list of significant works that topple prevailing scientific certainties. If Arp is correct in his general conclusions—and there are strong indications that he is—then he may eventually enter the history of astronomy as one of the most important heretics since Galileo. Paradigms that appear in serious danger of being shattered, thanks to the monumental work of Arp and a handful of other astronomers, include at least these: that the universe emerged from a single Big Bang expansion of space-time some ten to fifteen billion years ago; that the redshifts of distant objects really do indicate a cosmological expansion of the universe and that late twentieth century theories of force unification at the "beginning of time," string theories, and that a host of hyperdimensional mathematical speculations are valid. If Arp is right about cosmology, these theories might be the nonsensical equivalent of planetary epicycles, cosmo-babble that persisted for almost two thousand years until being overthrown in the 1600s in the Copernican revolution and by telescopic observations.

Arp possesses stellar credentials, though these do not make him right any more than do those of his equally well-credentialed critics. He received his bachelor's degree from Harvard College in 1949 and his Ph.D. from Caltech in 1953, both *cum laude*. For twenty-nine years he was a staff astronomer at the observatories known originally as Mt. Wilson and Mt. Palomar. He has been on the staff of the Max-Planck-Institute for Physics and Astrophysics in Munich, Germany. His exposition is precise, coherent, and at times deliciously well-written. The books are reasonably accessible to those without astronomy backgrounds, although some clarifications for those less experienced with astronomical observations might have helped here and there.

Arp has compiled exhaustive evidence about mysterious objects, called quasars ("quasi-stellar objects"), the first one of which was discovered in 1963. Since the mid-1960s it has been nearly universally accepted that quasars are at gigantic, *i.e.* cosmo-

laser continues on page 36

logical, distances from the Milky Way galaxy, home of the Sun. Their apparent brightness, interpreted for those huge distances, implies fantastic inherent brightness for quasars, or so say establishment astronomers. Arp maintains that quasars are *not* at huge cosmological distances; he believes the evidence is overwhelming that they are associated with galaxies that are much nearer than the redshift in the spectral lines of their light would imply, according to conventional universe expansion theory. Therefore, according to Arp, quasars are not inherently spectacularly bright or energetic. Sacred cows of cosmology are seriously demoted.

Arp's primary *modus operandi* has been to find quasars—often in pairs, but sometimes not—which appear to be *angularly* very close to a galaxy. Their redshift, interpreted as part of the purported Hubble cosmological expansion, would imply that they are background objects—far, far away from the galaxy to which they are adjacent in the sky. The light of the galaxy that Arp claims is *functionally* (not just angularly) associated with the quasars has a much lower redshift. Therefore, it is much closer to us than the adjacent quasars. Arp has provided mountains of solid statistical data that point to the virtual impossibility that such quasar-galaxy associations and alignments are merely coincidental—accidents of large number statistics. He provides compendious evidence that quasars are actually ejected from cores of the active nuclei of nearby galaxies and thus, *of course*, appear to be near them. Some of these quasar pairs are very well-aligned—collinear—with the parent galactic core. Radio telescope and X-ray/gamma ray satellite observations in some cases indicate “bridges” of emitting material linking some quasars with the galaxy from which they apparently come. This is the smoking gun that proves Arp's position, but it seems that the astronomy establishment does not see what it doesn't want to see. The mathematical probabilities that some of the alignments and other associations of quasars and active core galaxies are purely accidents of geometric perspective are, in Arp's view, less than 10^{-8} to 10^{-6} !

Entirely different physics—other than supposed Hubble-expansion redshift—would need to be invoked to explain how quasar light could be so remarkably redshifted while those objects were no further away from us than the central galaxy with which they seem to be associated. What we have, it seems, is evi-

dence screaming at us from the heavens that Big Bang cosmology may well be a house of cards ready to fall—and along with it “nearly complete,” so-called “theories of everything” in physics.

That Arp is not being taken seriously by the astrophysical establishment, that he is, in fact, a heretic outcast, as his scientific and human account so well document, is because contemporary astronomers refuse to pay attention to seemingly irrefutable photographs, and associated X-ray and radio telescope maps. These show that the Arp opponents might not be just wrong but *catastrophically wrong*. There is a cognitive dissonance between what has been deemed almost universally true—that galactic redshift is due to cosmic expansion—and the now *unthinkable* notion that redshift could mean something entirely different. It may be the twentieth century's equivalent of the Church fathers not being willing to look through Galileo's telescope. How ironic.

In Arp's *Seeing Red* (SR), aptly titled for dual meaning, he concludes:

More than ten years have passed, and in spite of determined opposition, I believe the observational evidence has become overwhelming, and the Big Bang has in reality been toppled. There is now need to communicate the new observations, the connections between objects and the new insights into the workings of the universe—all the primary obligations of academic science, which has generally tried to suppress or ignore such dissident information.

Seeing Red (1998) and the earlier *Quasars, Red Shifts, and Controversies* (1987) beautifully blend exposition of the scientific subject matter with presentation of the socio-pathology of the data-resistant critics. It describes how they have gone about their dirty work of wantonly rejecting papers and cutting off telescope observation time to the quasar heretics, while providing no substantive reasons for such attacks. His first redshift book was turned down by “uncountable numbers of publishers,” until tiny Interstellar Media in California, headed by Donald Goldsmith, took up the challenge. In SR, Arp articulates what a cold fusion researcher might well say about his colleagues:

Those friends of mine who also struggle to get the mainstream of

Seeing Red continues on page 34



Figures reprinted with permission of Hilton Arp.

Figure 1-3. Seyfert galaxy NGC4258, photographed in the light of hydrogen alpha emission, showing excited gas emerging from the nuclear regions (P. Roy *et al.*). [Ed. note: The two associated quasars are about equidistant from the galactic nucleus. One quasar has a redshift (z) of $z = 0.40$, the other $z = 0.65$, while the galaxy's redshift is only $z = 0.002$. The quasar alignment is within 3° of 180° to either side of the galaxy. The grand probability of this quasar-galaxy picture being an accident of geometric perspective is less than 4×10^{-7} , according to Arp. From page 13, *Seeing Red*.]

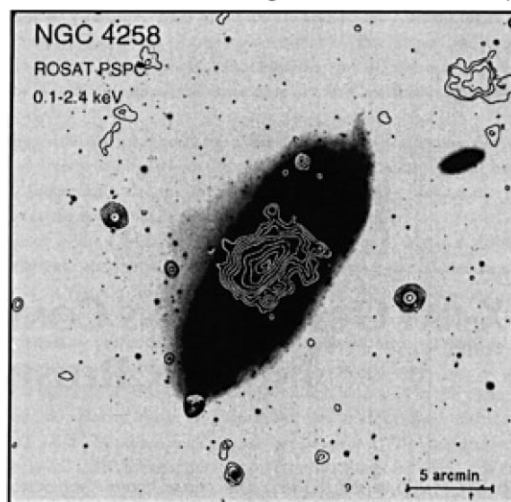


Figure 1-1. The Seyfert galaxy NGC4258 is known to be ejecting material from an active nucleus. A deep photograph is shown here with contours of X-ray emission superimposed (W. Pietsch *et al.*). The two point sources on either side of the nucleus coincide with blue stellar objects (BSO's). [Ed. note: Arp wrote that the BSO's are the quasars of Figure 1-3. From page 10, *Seeing Red*.]

Watch for Jed Rothwell's review of *Excess Heat* in the next issue of *Infinite Energy*.

An excerpt follows from *Excess Heat*, a new book by Charles G. Beaudette that explains why there was, and is, so much confusion in this field called cold fusion. Ordering Information - See page 5 and 72.

• **Failed Experiments** • What ought to be made of the many failed attempts to generate excess heat energy? These experiments created no excess heat, no neutrons, nor any other nuclear product. Many scientists took pains that their experiments were designed according to the best information available. They were carried out exactly as reported in their published papers so far as is known. Did such experiments demonstrate that there was no "cold fusion" phenomena of interest? Did those failed experiments invalidate the claims of Fleischmann and Pons? How was their significance to be weighed?

A large fraction of the unsuccessful experiments were run by scientists who also had successful experiments. These failed experiments were reported by scientists who had measured anomalous power with their own hands. They had developed confidence in their techniques, and they accepted anomalous power generation by some of their cells as a real phenomenon. They concluded that there was some additional agent (a variable or parameter) in the failed experiment that was not under control. The failed experiment may be of value to the experimenter.

Many experimenters never saw a positive result, even after intense effort. These included some of the most prestigious institutions such as MIT, Yale (at Brookhaven), Caltech, and Harwell (England). Does the caliber of scientists and resources that such institutions can bring to bear on a task imply that their failed results are the correct results? . . .

A constructive way of looking at the failed cold fusion cells is as follows. Imagine you have a lake before you, and you want to know if there are fish in it. You send out one hundred expert fishermen to fish. When they return, ninety-five of the fishermen have caught no fish, and five fishermen are each holding up a fish. Now your question can be answered.

The political answer to the question is to take a vote, as the APS did for its third press conference on May 2, 1989, at Baltimore. Clearly, if you do that, the "no fish" have it: a ninety-five to five vote says that the lake does not contain any fish. Or, one can use scientific reasoning and argue that the five fish caught indicate that the lake does contain fish. It may also be argued, of course, that the five fish were the result of fraud or incompetence, *e.g.* that the five caught actually were smuggled in a tackle box or that they were eels, not fish. In any event, the claim that a fish was caught is at the center of the argument. Counting the ninety-five empty returns is of no help. Similarly, counting the number of failed cold fusion experiments is of no diagnostic value.

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Book Reviews

Seeing Red continued from page 33

astronomy back on track mostly feel that presenting evidence and championing new theories is sufficient to cause change, and that it is improper to criticize an enterprise to which they belong and value highly. I disagree, in that I think that if we do not understand why science is failing to self-correct, it will not be possible to fix it.

Bravo!

Arp offers this plea:

The only hope I see is for the more ethical professional and more attentive open-minded nonprofessionals to combine their efforts to form a more democratic science with better judgement, and slowly transform the subject into a more enlightened, more useful activity of society. This is the deeper reason I wrote this book and, although it will cause distress, I believe a painfully honest debate is the only exercise capable of galvanizing meaningful change.

There are huge issues at stake in the basic science that Arp lays out in SR,

. . . if the cause of these redshifts is misunderstood, then distances can be wrong by factors of 10 to 100, and luminosities and masses will be wrong by factors up to 10,000. We would have a totally erroneous picture of extragalactic space, and be faced with one of the most embarrassing boondoggles in our intellectual history.

That is an understatement.

There were warnings earlier, even by the very man for whom the supposed redshift-distance (or redshift-apparent magnitude relation) for galaxies was named, Edwin Hubble. ". . . Hubble, the observer, even up to his final lecture before the Royal Society, always held open the possibility that the redshift did not mean velocity of recession but might be caused by something else." This has conveniently been forgotten in the torrent of media-acclaimed evidence for the Big Bang and the supposed certainty of cosmic expansion. Arp's retrospective:

After about forty-five years, I now know that if the academic theoreticians at that time had not forced his (Hubble's) observations into fashionable molds, we might at least not have started off modern cosmology with the wrong fundamental assumption. We could be much further along in understanding our relation to a much larger, older universe—a universe which is continually unfolding from many points within itself.

As in other areas of heretical research, such as cold fusion, critics seem to have abandoned reason, or taken leave of their senses, in their passionate quest to kill the offending heresy. Even if Arp were wrong about his interpretation of the data, their behavior would be inexcusable. Arp notes that one critic told him, even in the face of overwhelming mathematical proof of the statistical significance of certain observations: "No matter what you do, you will never be able to prove that you are right. If you are right, it will have to be proved by someone else." Arp observes:

What has happened in astronomy today, however, is that almost all telescope access has been shut off to the proponents of one point of view—namely the point of view that current assumptions should be subject to observational test and that contradictory and surprising evidence should be followed up.

Sounds familiar!

Though Arp stays mainly with explaining observations, their meaning, and the process by which they have been rejected or ignored, he does put forth in SR an intriguing and plausible general cosmology:

As a capsule view of how galaxies are born, we can say that they are ejected from older galaxies as compact objects with low particle masses. As these newer galaxies age, and grow in size and mass, they in turn eject newer generations in a cascading process. We can actually show in Chapter 8 how groups of a dozen or so active quasars fraction into more and more objects, which in turn eventually evolve into clusters of large numbers of galaxies. The redshifts, which are very high as the newly created matter emerges from its zero-mass state, continue to diminish as the mass of the matter grows. Discrete steps in the redshift values are present throughout, but grow smaller when the overall redshift grows smaller. These aggregates of matter develop into normal galaxies, much like our own and those around us in the Local Group and Local Supercluster. All of this is almost diametrically opposed to the conventional view of galaxies condensing out of some tenuous, homogeneously pervading hot gas. It is a process that is going on in our own Local Supercluster, and, contrary to what is claimed by the Big Bang theorists, we do not know much about what may exist at cosmic distances.

There is much more to support this view. For example, Arp presents the significant evidence that quasar redshifts are quantized, *i.e.* fall near discrete values. He writes,

The fact that measured values of redshift do not vary continuously but come in steps—certain preferred values—is so unexpected that conventional astronomy has never been able to accept it, despite overwhelming observational evidence. . . extragalactic redshifts, in general, cannot be velocities. . . the whole foundation of extragalactic astronomy and Big Bang theory is swept away.

An added feature of SR, which may please many *Infinite Energy* readers, is the chapter titled "Academia." Arp runs through the dynamics of a number of scientific controversies that bear some resemblance to the one at hand. He even addresses cold fusion briefly, noting that it has faced the kind of *a priori* dismissal that his cosmic evidence has encountered.

Both these books are gems, though SR is more up-to-date. They deserve to be read carefully and pondered by all who seek the truth about the fabric of the heavens. What we see of the universe may continue to evolve in most surprising ways. Matter, and thus energy, may continue to be created in galactic cores (*ex nihilo*?), and who knows where else. The data that heralds this awareness has been lovingly compiled and offered to a wide audience by a very heroic author with whom we must feel kinship. □□□

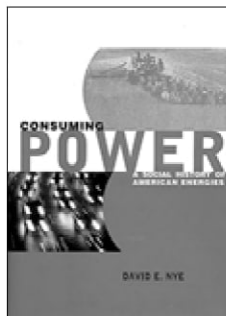
Consuming Power: A Social History of American Energies

by David E. Nye

ISBN 0-262-64038-4

\$17.95 Paperback, 352 pp.

MIT Press, 1998



Review by Susan Seddon

As a high school student I would never have voluntarily read a book like this one on the perfectly legitimate grounds that it had hardly any pictures, it was block after block of unbroken text, and there was no romantic interest. I was reminded of those halcyon days when I first glanced at the index—it certainly appears very school “text-bookie”—but perhaps it wouldn’t be so comprehensive if written any other way.

Comprehensive it certainly is, providing an in-depth and cohesive commentary on the

way the availability of energy has influenced social development in the United States from the days of muscle, water, and steam power, through fossil fuel days of feast and famine, to the nuclear and computer age. The author’s main point, that over the decades “energy choices are social constructions that appear inevitable once they acquire technological momentum,” is well made.

One recurring theme which I found particularly striking is the way the adoption of successive energies has increasingly led to social isolation. Home-working and small-scale, local trading between craftspersons in the muscle/waterpower age became obsolete with steam and fossil fuel driven factories and the adoption of centralized production. The consumer no longer personally knew who had made the goods he purchased and, with the simultaneous emergence of faster and improved transport systems, he couldn’t be certain even where in the world it was manufactured.

The effects of energy abundance changed the American way of life completely with self-sufficiency being replaced, within a couple of generations, by a powerful desire for acquisition, when often possession of a television or a motor car ranked more highly than effective household plumbing. The United States swiftly became a consumer society which assumed an energy surplus and even equated success and happiness with the control of large amounts of energy.

In the period from 1920 to 1970, demands for electrical power doubled each decade, and great wonders were envisaged for the dawn of nuclear power in the 1950s, although by the end of that period, thanks to the abundance and cheapness of fossil fuels, atomic power still supplied less commercially usable energy than firewood. “Hot” fusion power was first heralded as the energy of the future in the mid-1960s when the World’s Fair guidebook claimed it to be a “source of energy great enough to last for billions of years with the oceans of the world serving as an inexhaustible reservoir of fuel.” A specially-made film showed the awesome potential of fusion power—a flash bang lasting six millionths of a second and requiring only 100 million degrees temperature!

Before Iran and the OPEC embargo, sociologists fretted that the greatly reduced requirement for skilled manual labor would result in a leisure crisis where inactivity led to extremism and intolerance. They forgot, however, to include human greed in

their equations. . . it seemed like however much people had, they always wanted more. The throwaway society had come of age and only disasters like large oil spills and Three-Mile Island prompted the idea that there could be a price to pay for this golden age of energy abundance. Nye concludes that there are still many hard choices to be made.

The book doesn’t mention cold fusion at all, which I found disappointing, as the 1989 announcement held all the promise of the true American dream of limitless energy much more so than any previous technology. Speculations on the social impact wrought by cold fusion technology alone would undoubtedly fill another book equally as weighty as this one. Nye is right, however, in stating that whatever form future energy takes, it will only happen if, as in the past, the consumer himself chooses to make it so. □□□

Review by
Jeffery D. Kooistra

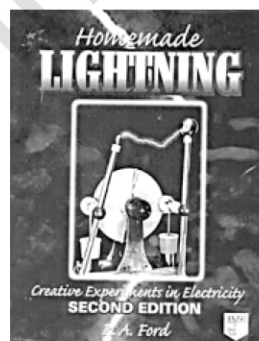
Homemade Lightning: Creative Experiments in Electricity

by R.A. Ford

ISBN 0-07-021528-6

\$19.95 Paperback, 223 pp.

TAB Books, The McGraw-Hill Companies, Inc., 1996



Homemade Lightning is now available from Cold Fusion Technology.
\$21.95 North America
\$25.95 Foreign (Includes Postage)

Homemade Lightning is both an excellent first book for the budding electrical experimenter and a superb book for accomplished experimenters who haven’t spent much time with electrostatics. Richly illustrated with diagrams and pictures, *Homemade Lightning* abounds in specific construction details for a wide variety of electrostatic machines—some simple, some complex, and some you’ve never heard of.

One delight of the book is Ford’s overall philosophy. Though not dogmatic about conventional theory, neither is Ford given to flights of fantasy. He points out where past research both went, and where it didn’t go, and suggests many places where

original work needs to be done. As he says in his introduction, “[T]his book is not written for the couch potato who waits passively to be entertained, but rather for those who love to build, experiment, and investigate along original lines of research. I am especially interested in encouraging high school students and their teachers who want to do something really unusual for their science fair projects.”

Ford also includes short but intriguing chapters on unconventional electrical topics, among them “Electrohorticulture,” “Electroaerodynamics,” and “Unusual Electric Discharges.” These chapters are both entertaining and suggestive of “things as yet unknown.”

Ford also says, “Although high-voltage electricity does have great entertainment value, my main objective is to revive it as a serious tool for physics research.” A laudable objective—we at *Infinite Energy Magazine* couldn’t agree more. □□□

**Science and Human Transformation:
Subtle Energies,
Intentionality and
Consciousness**

by William A. Tiller and
Ernest F. Pecci

ISBN 0-964263742

\$24.95 Paperback, 316 pp.

Pavior Publishers, 1997



Review by Mike Carrell

Tiller's book is systematic work to bridge the gap between the apparent certitude of prevailing physical theory and the considerable body of traditional wisdom and anecdotal experience which says there is something more to human existence than chemistry and mechanics. The gap is partly a matter of language and paradigms, but also of direct experience. Tiller is one of a relatively small group with respected credentials in the physical sciences (Professor of Material Science & Engineering, Stanford University) who also actively participates in personal explorations of other modes of consciousness. Thus, his discussion is one of incorporation, not exclusion.

He begins this book by describing experiments with

simple devices that interact with human perception and intention in anomalous ways. In a gas cell with a high voltage impressed, micro-avalanche discharges can occur without breakdown into arc or glow discharge; in this operating region, the rate of micro-avalanches responds to the focused intention of people. Physiological indicators of a human subject can be influenced by the intention of a person in another room. The infrared transmission spectrum of water can be altered by the intention of a healer and can persist after being removed from the healer's hands. In the region between electrodes held at high potential, some children see complex luminous patterns not seen by adults. There are more, some performed by Tiller, some by others, from a bibliography of thirty-four citations.

All these might be dismissed *a priori* because they do not fit accepted models. He then proceeds to a well-informed discussion of the foundations of physics, which includes models of space-time expressed as matrices of parameters describing the state and direction of existence at any location. Einstein used a four-by-four matrix to describe general relativity, others have extended the notion to larger and larger arrays. Tiller notes:

The important thread running through all these hyperspace considerations is that the laws of nature appear simpler from a higher dimensional perspective. It is as if, for a very large N , (number of dimensions to the matrix) there is only one law, perfectly symmetrical and perfectly beautiful, and that a cascade of sequential symmetry breakings yield the multiplicity of seemingly disparate laws that govern our physical level observational experience.

Only a small portion of such a universal matrix need be assigned to electromagnetism and the nuclear binding energies; other parameters may be assigned to what Tiller calls subtle energies, which are wave-like, yet may have superluminal propagation velocities. These do not interact with electromagnetic instruments, but may interact with living systems. Tiller applies the principle of the Fourier transform to these subtle energies, discovering that time-serial events can be transformed

into spectra, images to patterns, in a manner well-known in electromagnetism, including radio and optics.

He draws a thread suggesting that patterns, such as mandalas, perceived subjectively in meditation states by some people may be a kind of Fourier transform of subtle energy flux in the universe or between individuals. To go further in a brief review would be an injustice to Tiller's considerable effort. Glimpses of parts of the very rich landscape Tiller explores may bring an impulse to reject some feature and miss the integration to which he invites the reader. He brings within a consistent framework an extremely wide range of experiments with people which indicate interactions that do not fit within narrow interpretations physics and simplistic electrochemical models of living organisms. Tiller is not alone in his quest. There is a community of people seeking an understanding of the mind-body problem in quantum physics, non-locality experiments, and the like.

To get a full appreciation of Tiller's work, it is necessary to walk with him. It will be a journey strange to many, for the landscape includes auras, chakras, and other manifestations not visible to most humans. The back cover carries an endorsement by Ingo Swann, who has demonstrated remarkable psychic abilities under controlled conditions. Another endorsement by C. Norman Shealy, MD, praises Tiller for setting in motion a new paradigm—integrating previously disconnected pieces of the human health puzzle.

□□□

First they ignore you. Then they laugh at you. Then they fight you. Then you win.

— Gandhi

laser continued from page 32

history, based on emotions, with no deep cause. Cold fusion scientists must work to calm down the hysteria at the APS, just as dentists had to assure the public that fluoridation would not lead to mind control from Moscow.

At age 77, Margaret Sanger bullied reluctant biomedical researchers to follow up on thirty-year-old discoveries and persuaded Page McCormick to fund the work. She had spent a lifetime crusading for unpopular causes. At an age when most people retire, she was launching a biomedical and social revolution. One determined person can sometimes overcome opposition from millions of people, and from the church and government establishments. It is inspiring, yet frightening. Apparently, great ideas need an extraordinary person to fight for them. Many ideas may have been lost without a Margaret Sanger or a Thomas Huxley to champion them. (Huxley defended and popularized Darwin's theories and became known as "Darwin's bulldog.") Townes continued to work on the maser after two Nobel laureates, one of them his boss, walked into his office and told him to stop. How many scientists would have the guts to do that? How many discoveries have been pushed aside because world-class scientists like Rabi, Kusch, and Bohr discouraged younger colleagues or ordered them to stop?

□□□



COMPILED BY EUGENE MALLOVE



Will the President Phone Bow?

In the last issue of *Infinite Energy*, we announced that in February the White House had telephoned us in Bow, New Hampshire to ask Editor-in-Chief Eugene Mallove to submit a briefing memorandum on cold fusion/new energy. We reported that the 8,000 word manuscript plus Executive Summary, "The Strange Birth of the Water Fuel Age: The Cold Fusion 'Miracle' was no Mistake," was to be included in the President's book, along with some forty other essays about the future, submitted by notables and the not-so-well-known. Among the recommendations set forth in the essay was this key one at the end of the Summary: "Mr. President, you need do only one thing now: Publicly state that you are going to investigate this matter and then do it."

On April 3, after he inquired about the status of his submission, Dr. Mallove received this email message from one of the officials in charge of the briefing book project:

Dear Dr. Mallove:

I assure you that it [the briefing memorandum] is somewhere very close to him [President Clinton] if not on his desk at the moment. The process has been quite a slow one—I'm glad that it has finally come to the completion stage.

I am nearly certain that the President will take a moment to reach out either by mail or by phone to all of the contributors to this project.

We hope to hear from the President soon about the project.

Thank you for contacting me.

Yours,
Shawn Johnson
Special Assistant
Office of Communications
The White House

We are delighted that the process has come so far along. We await a possible telephone call or letter from President Clinton, in which we hope to learn his reaction to our carefully honed message.



Vice President Al Gore "Very Interested" in Cold Fusion

Infinite Energy this April gained insight about peripheral interest in cold fusion at a high level within government.

A subscriber, who wishes to remain



"...it merits
thorough
consideration."

anonymous, sent Vice President Al Gore a copy of our video documentary "Cold Fusion: Fire from Water." The Vice President sent back a nominally *signed* letter to the subscriber, in which he states that he thinks that cold fusion is "a very interesting issue" and that "I agree that it merits thorough consideration."

We have seen a copy of the letter and would have published it in its entirety, had this loyal subscriber requested that we not do so. We respect his right of refusal, but we could not fail to report to our readers that the Vice President of the United States in the summer of 1999: 1) had a copy of our cold fusion documentary in his possession and 2) stated that cold fusion "merits thorough consideration." We have no idea whether Al Gore actually watched the tape, but we certainly wish that he would raise the issue publicly and investigate it properly, much as we have asked President Clinton and many other presidential candidates to do.

People who never change
their minds are either perfect
or stubborn.

Submitted by Walter S. Rosenthal



James Randi Educational Foundation to Publish Mallove's Review of Robert Park's *Voodoo Science*

Eugene Mallove's scathing review of Robert Park's forthcoming book, *Voodoo Science: The Road from Foolishness to Fraud*, which appeared in our last issue (pp. 44-45, 68) is being reprinted (with our permission) by *Swift*, the newsletter of the James Randi Educational Foundation.

The American Physical Society's PR agent Dr. Park has a regular column in that newsletter. James Randi himself has been no friend of cold fusion, having made several rather "impolite" remarks about it in the past (see *IE*, No. 20, pp. 41-42). So it was heartening to learn that Randi's skeptical audience would be exposed to an analysis of how wrong-minded Dr. Park has been about cold fusion (and a few other notable topics). Park asserts in *Voodoo* that cold fusion became *fraud* by June 1989, even though he mentions none of the published scientific confirmations that have occurred, especially the solid results on helium production in cold fusion cells—evidence that he demanded in 1989-90.

Swift quite appropriately offered Park space to rebut Mallove's review of *Voodoo*, which occupies four pages of the *Swift* issue, but this is all Park could offer in response: "I am delighted that *Swift* has chosen to reprint the Mallove review of *Voodoo Science*. My only concern is that readers unfamiliar with Mallove might suspect it of being a hoax intended to promote my book. I mean, who would believe there is actually a magazine with the title *Infinite Energy*?" To that we say, "Checkmate, Robert Park! You offer no serious reply to the charges that you have been bamboozling audiences about the status of cold fusion research."

Sulfur-Deprived Bacteria Produce Hydrogen

The biggest obstacle preventing conventional "hydrogen economy" from breaking out is the lack of natural free-

hydrogen sources on Earth—no hydrogen “mines” or “wells” exist here. (In the core of Jupiter, maybe, but not on Earth!) Hydrogen used in fuel cells or internal combustion engines must be obtained from water or from naturally occurring hydrocarbon fuels. Though water is by far the preferred source of hydrogen, conventional technology requires that more energy be expended to obtain hydrogen from water than can be recovered in today’s fuel cell or combustion processes. All these rely on conventionally understood chemistry, not on the immensely more energetic low-energy nuclear reactions (cold fusion) and the like.

Enter the lowly green bacterium, *Chlamydomonas reinhardtii*. The January 2000 issue of *Plant Physiology* reports the work of Tasios Melis of the University of California at Berkeley and his colleagues at NREL (National Renewable Energy Laboratory) in Golden, Colorado. These researchers have found a way to make this green algae produce hydrogen in larger quantities than previously seen. Though the dream of using solar-powered microbes to generate hydrogen has been around for decades, hydrogen yields from bacterial tanks have not been especially high. Even with this newly announced process, yields are still low, perhaps only a few milliliters of H₂ per hour from one liter of algae suspension.

The new trick found by the researchers is to deprive the algae of sulphate salts. This blocks the oxygen-producing photosynthesis pathway. The produced oxygen would normally combine with any cell-produced hydrogen to form water. The serendipitous discovery is that hydrogen alone is produced by the cells under sulphate deprivation. After four days or so of hydrogen production, the cells must be allowed to resume their normal cycle, which relies on sulphates being present. Later, the cells can be turned on again to their hydrogen-producing mode.

Though the economics of this solar energy-driven microbiology process are not yet clear, the researchers express hope that it could be made much more efficient. As reported in *Science* (March 3, 2000, Vol. 287, p. 1581) another group at Oak Ridge National Laboratory employs a different process for getting around the oxygen production problem—flushing the algae system with nitrogen. For two months the Oak Ridge process equaled the production level of the sulphate-deprivation process. Making these microbial processes even ten times more efficient would still require very large

“pond” areas to run powerful engines. Though these “power from pond scum” processes were widely touted in the news media, they do not appear to be, even in principle, the kind of revolutionary methods that could revolutionize the energy economy. A pond area the size of New Mexico, for example, might be required to produce enough hydrogen to power the United States at today’s consumption level, according to one NREL researcher.



New Energy Employment Ads

The February 28, 2000 issue of *Chemical & Engineering News* and the March 3, 2000 issue of *Science* each have half-page employment advertisements for technical staff at BlackLight Power, Inc. of Cranbury, New Jersey. The ads for the new energy company, which in February was awarded a U.S. Patent for the catalytic hydrogen energy process of Dr. Randell Mills, are bannered: “Be a Part of the Chemical and Energy Revolution.” The staff being sought include: Ph.D. level Analytical and Inorganic Chemists; Analytical Instrumentation Research Assistants; Scientists and Engineers; Technicians; Battery Scientists and Engineers and Electrical Engineers.



Hot Fusion Happenings

What next for the hot fusion program? The U.S. Department of Energy’s proposed FY 2001 budget for “Fusion energy sciences” is \$247.3 million (the approved FY 2000 budget was \$244.7 million). This covers the magnetic confinement fusion part of the hot fusion budget. Inertial confinement (laser) fusion is lumped under DOE’s “Weapons activities” part of the budget—\$1.953 billion requested for FY 2001. This includes the already cost-overrun National Ignition Facility (NIF) at Lawrence Livermore National Laboratory. To put this in perspective, the total DOE budget sought for FY 2001 (\$18.9 billion) is up some 9% from FY 2000 authorization.

The magnetic confinement hot fusion people have continued to orchestrate their “comeback,” following the 1998 Congressional killing of funding for U.S. participation in ITER (International Thermonuclear Experimental Reactor). After

the U.S. departure from the original over \$10 billion collaboration of U.S., Russian, European, and Japanese scientists to build a 1,500 megawatt, 31.5 meter-high tokamak fusion reactor, it was scaled back. It became “only” a \$3 billion dollar experimental device that would generate 400 megawatts of fusion energy. The magnetic fusion scientists had hoped to go to “ignition”—50 times the input electrical power—but their scaled back plans for ITER call for only a 10/1 gain. Mind you, these projected power ratios would last only for relatively short times during an experiment.

A March 2000 *Scientific American* News and Analysis article, “Burning Times for Hot Fusion” (pp. 19, 22) concludes, “For ITER supporters, the immediate concerns remain political, such as agreeing on a country to host the reactor and getting sufficient funds.” The article reports that ITER could be built in fifteen years and “see results within twenty-five years.” U.S. ITER supporters hope that the continued reports of European and Japanese support will draw the U.S. in again. Yes, this form of hot fusion certainly seems to be “the energy source of the future—and it always will be.” Electrochemical cold fusion/excess energy processes achieved power break-even in the mid-1980s and in the early 1990s experimental “ignition” or “heat-after-death,” self-sustaining power production. Yet the U.S. DOE continues to ignore and/or attack cold fusion while spending huge quantities of taxpayer money on a white elephant that is never likely to achieve its vaunted goals.



BlackLight Power, Inc. Issues Warnings

Since 1991, the interminable assaults by American Physical Society’s flak, Dr. Robert Park, have encouraged several mainstream physicists to make rather nasty remarks about the commercial activities of BlackLight Power, Inc. and its scientist founder/CEO Dr. Randell Mills. Recently some of these negative remarks were highlighted in a *Village Voice* article about Dr. Mills and his company (*IE*, No. 29, pp. 40-41). The negativity from the mainstream did not stop the U.S. Patent Office on February 15, 2000 from issuing patent No. 6,024,935 to Dr. Mills (assigned, of course, to BlackLight Power). The patent, “Lower-Energy Hydrogen Methods and Structures,” covers catalytic power generation

through the creation of purported below-ground-state hydrogen atoms, "hydri-
nos." The mainstream physicist "buzz"
against Dr. Mills' hydrino theory and
commercial activity has apparently led to
unexpected difficulties for BlackLight in
its efforts to protect its intellectual prop-
erty. Well-informed sources have told
Infinite Energy that BlackLight is pursuing
a legal investigation of the matter, mainly
because of a notable patent process break-
down. It is noteworthy that despite years
of insults that Dr. Park and his colleagues
had heaped upon Dr. Mills and his sci-
ence, no previous legal remedies had
apparently been sought. This appears to
bolster the claim of the informed source
that a serious impact on the patent
process has occurred, which might justify
legal investigations and perhaps more.

Dr. Park, spokesman for the APS, now
publicly protests that BlackLight Power is
attempting to muzzle mainstream physi-
cist critics by threat of legal action. A
number of the offending scientists have
received letters asking them to moderate
their public statements about BlackLight
Power. Princeton University Physics pro-
fessor and Nobel laureate Phillip Ander-
son, for example, was quoted in the *Village
Voice* article: [if Mills' theory were correct]
"...everything we know about everything
would be a bunch of nonsense. That's
why I'm so sure that it's a fraud."

Nature magazine, March 16, 2000 (Vol.
404) has an article, "New Form of Hydro-
gen Power Provokes Skepticism," about
the recent legal thrusts by BlackLight
Power. In the article by Tony Reichhardt,
Park asserts disingenuously: "...the issue
is not whether their [BlackLight Power's]
stuff is out there for review. The issue is
whether anybody believes it and whether
people who don't believe it have a right
to say they don't believe it." Park knows
very well that for years he and like-mind-
ed scientists have criticized and insulted
the scientific claims of Mills with impu-
nity. Park does not admit here any direct or
indirect interference with the patent
process. Yet in the *APS News* of March
2000, which was distributed at the recent
APS meeting in Minneapolis, a news
story about Park and his forthcoming
book *Voodoo Science* boasts about Park's
long reach: "... Park's efforts to expose
the fraudulent claims of free energy
schemes—a movement which has
achieved nearly cult-like status—led to
the removal of State Department spon-
sorship of a free energy conference last
April, and an investigation of the Patent
and Trademark Office, resulting in the
dismissal of the U.S. patent examiner

who organized the conference." (See *IE's*
account of the COFE meeting, No. 26, pp.
10-15; and the USPTO's firing of COFE
organizer Thomas Valone, No. 28, pp. 46.)

In the tradition of *Nature's* non-cover-
age of cold fusion and new energy, its
article on BlackLight of March 16 discuss-
es no scientific claims, data, or theories—
other than to disparage those peer-
reviewed journals in which such infor-
mation can be found! However, it does
offer an unflattering cartoon of a
spokesperson for BlackLight Power, Inc.
(intended to be Dr. Mills?) telling the
press, "A single hydrino can produce
enough energy to keep an expensive law
firm running for a year!"

Late Breaking News: See new develop-
ments in this story in a *Village Voice* article
by Erik Baard, www.villagevoice.com
—April 26-May 2, 2000.



Climates of Change Congress

From March 19 through March 22, 2000
in beautiful Victoria, British Columbia, a
few hundred attendees and several dozen
speakers met at the first "Climates of
Change Congress: Mapping the Transi-
tion to a Sustainable Energy Future." The
meeting was held at the Victoria Confer-
ence Centre. A diverse group of speakers
addressed topics from "atmospheric sci-
ences to innovative environmental and
social programs; from breakthroughs in
energy source development to new con-
cepts for economic and financial prac-
tices." Of particular significance to *Infinite
Energy* readers was the Tuesday afternoon
program (March 21), which featured an
"Alternate Technologies" session. Hal Fox
spoke about New Energy Solutions;
Eugene Mallove addressed Cold Fusion;
Jeane Manning gave a talk, "A Technol-
ogical Plan for Reducing Global Warming,"
and Mark Comings spoke about Zero
Point Energy. Noted futurist and author,
Hazel Henderson, opened the conference
with her speech, "Beyond Globalization:
The Politics of the Solar Age." Canadian
television outlets provided significant
coverage of the conference.

It is heartening to realize that a gath-
ering of "mainstream" environmental
activists and speakers has opened its
doors to learn about new physics solu-
tions to the world's energy and envi-
ronmental problems. May we have
more of this!



Iridium L.L.C.

One of the most extraordinary high-tech-
nology, high-risk projects in history has
failed. Iridium L.L.C., the ambitious,
advanced low-orbit satellite communica-
tions system, is bankrupt. It will be liquidat-
ed within a few months. It cost Motorola
and other corporations more than \$5 billion
to build. The network needed millions of
subscribers to make a profit, but it never
attracted more than 55,000. The destruction
of the 74 satellites now in orbit will cost
another \$50 million. They will be de-orbited
to impact uninhabited areas. The ground-
based equipment is not compatible with
conventional satellite communications net-
works, so it will have little scrap value.

Shares in Iridium peaked at \$70 in 1998,
and closed at 66 cents recently. Other com-
panies have also lost money in the project;
Kyocera wrote off \$93 million. Another low-
orbit satellite network, ICO Global Commu-
nications, also filed for bankruptcy recently.

Technically, the system worked
according to specifications, but cus-
tomers were not interested in the service,
because it was too expensive and the
portable telephones were too large.

It is ironic that at the same time Motoro-
la was starting up the iridium project, they
dabbled in cold fusion—they were particu-
larly interested in the Patterson Power
Cell of CETI—but decided not to invest in
it even though they had gotten many pos-
itive results. Cold fusion would be a micro-
scopic risk compared to Iridium. Motorola
had the guts to do cold fusion, and had the
money. They lacked only the will.

Iridium was a failure of marketing, not
technology. As one commentator put it,
"Motorola bet too early on the wrong technol-
ogy. . . They zigged and the world zagged."

The trouble with a low-orbit communica-
tion network is that there is no way to scale
it up in steps while you watch customer
reaction. It does not work until everything is
in place and the system is open for business
and ready to serve millions of subscribers. It
is all or nothing. Cold fusion, fortunately,
can scale from one customer to four billion.
A developer can work to impress a few
small customers at first, establish credibility,
and then gradually work his way up to
impressing big customers.

Iridium was the pet project of Chair-
man Robert Galvin and Chris Galvin, the
CEO of Motorola and grandson of the
founder. This demonstrates the surprising
influence individuals have on some mod-
ern corporations.

—Jed Rothwell



NEW ENERGY UPCOMING EVENTS

Announcement Special Session on LENR/CF at ANS Winter Meeting

You are invited to participate in a special session on LENR/CF at the winter American Nuclear Society meeting, Washington, D.C., November 12-17, 2000. This session is sponsored by the Isotopes and Radiation Division (IRD) and is being organized by George Miley. Paper summaries of 450-900 words, are due by June 15, 2000. For format details and submittal information, see www.ans.org. Submit papers to Mohammad Modarres, Technical Program Chair, ATTN: Transactions Office, ANS, 555 North Kensington Avenue, La Grange Park, IL 60526, with a copy to George Miley at 100 NEL, University of Illinois, 103 S. Goodwin Avenue, Urbana, IL 61801. If you have questions, please contact George Miley at g-miley@uiuc.edu or (217) 333-3772.

Solar 2000 Solar Powers Life—Share the Energy

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25th National Passive Solar Conference
AIA Committee on the Environment Symposium
ASME International Solar Energy Conference
Midwest Renewable Energy Fair

For further information, contact:
American Solar Energy Society
2400 Central Avenue Suite G-1, Boulder, CO 80301

• www.ases.org •

Watch for coverage
of the ICCF-8 Con-
ference in the next
issue of
Infinite Energy.



Calendar

ICCF-8, Eighth International Conference on Cold Fusion

May 21-26, 2000
Lerici (La Spezia, Italy)

The Natural Philosophy Alliance Meeting

June 5-9, 2000
Storrs, Connecticut

Solar 2000

June 16-21, 2000
Madison, Wisconsin

35th Intersociety Energy Conversion Engineering Conference

July 24-28, 2000
Las Vegas, Nevada

American Nuclear Society Meeting

November 12-17, 2000
Washington D.C.

35th Intersociety Energy Conversion Engineering Conference

July 24-28, 2000

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•Conversion Technologies • Electrochemical
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Systems • Policy Impact on Energy • Renewable
Energy Sources • Thermal Management

For additional conference information,
contact the American Institute of Aero-
nautics and Astronautics Customer
Service Center at 1-800-639-2422 or
• custserv@aiaa.org •

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• **June 5-9, 2000** •

University of Connecticut, Storrs, CT
Registration Fee \$45

For more information contact:

Prof. Domina Spencer, Department of Mathematics,
University of Connecticut, U-9
Storrs, CT 06269

Renewable and Conventional Energy

Lighting a Path to Distribute Renewable Power to the Third World

by Bennett Daviss

Researchers struggle to master the secrets of new energy sources and processes, but their work is only part of the solution to the world's energy crisis. Equally urgent is the search for effective new ways to finance and distribute renewable, decentralized energy systems—especially among the Third World's estimated two billion people who live on less than \$1 a day and the three billion living without electricity.

The need is dire: the poorer the population, the greater damage it does to the natural world—stripping it of trees to use for fuel and shelter and burning animal dung for heat instead of using it to renew soil.

In many areas south of the Equator, the absence of energy to run simple pumps consigns hundreds of thousands of people—most of them women and children—to spend hours each day hauling water. In those places, a localized source of energy to run a simple pump could free thousands of person-hours each month for more useful work. It also would ease the pressure on families to produce more children to share the chores.

A small nonprofit group based in Washington, D.C., has set out to solve this facet of the world's energy equation. The Solar Electric Light Fund (SELF) doesn't just dispense photovoltaic power systems in poor countries. It's also shaping a model of entrepreneurial, self-financing power distribution that can work with any decentralized energy source.

In its early projects, SELF used funds donated by the World Bank, private philanthropies, or loans from development agencies, to buy home-size photovoltaic systems in bulk on the open market, usually enough for one small village at a time. It then sold the systems at slim mark-ups to villagers in developing areas, usually forming a partnership with an in-country nonprofit agency. Each participating household made a 20% down payment on a system and paid off the balance—usually between \$300 and \$400—over several years. The buyers' payments were pooled in a local revolving loan fund from which their neighbors could borrow to buy their own solar power gear. SELF used a portion of the mark-ups on the equipment to establish a local dealership and trained local residents as solar installers and technicians.

The arrangement brought power to the

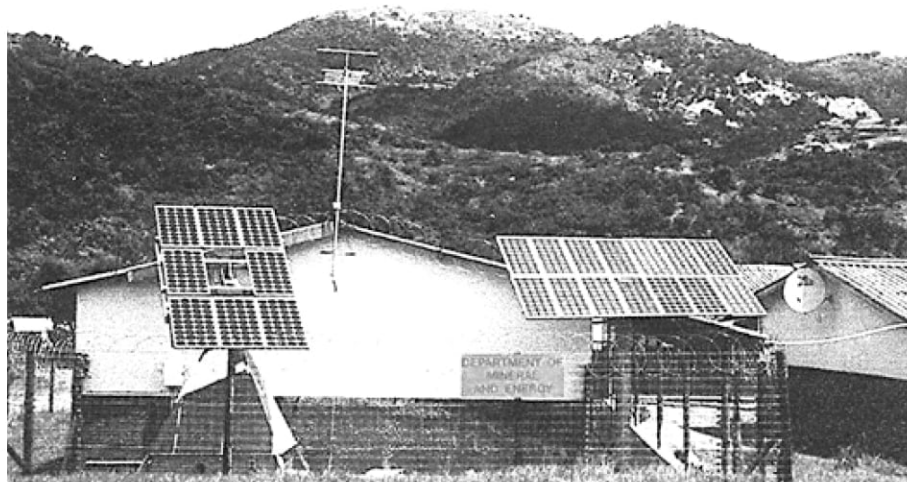
people in more ways than one. They had electricity for their homes and farms through equipment that they had paid for themselves. The technicians learned a profitable trade that also ensured that the power systems' continued operation didn't depend on return visits from outsiders with exotic knowledge. The loan fund made it possible for villagers to finance the continued dissemination of solar systems in their areas.

There have been broader benefits as well. In much of the developing world, the prime fuel for night lighting is kerosene. Although no agency keeps records, SELF estimates from anecdotal evidence that there are more than 20,000 kerosene-related injuries and house fires annually caused by spills and other accidents. In addition, every home burning the dim, kerosene-fueled lamps puts an average of six tons of carbon dioxide into the atmosphere annually and exposes family members to fumes as harmful as smoking two packs of cigarettes a day.

"Gone are the days when we have to spend up to \$50 each month for kerosene," one Solomon islander says, "and we no longer have fear because the electric current our equipment produces is safe."

Around the Equator, where darkness comes year-round by 6:30 p.m., children in PV-powered homes are able to study longer at night without eyestrain. And there is some evidence that when PVs power radios or small televisions in rural areas, birth rates fall: people have something else to do after dark.

SELF was founded in 1990 by Neville Williams, a former journalist who had promoted solar power as a staffer with the U.S. Department of Energy during the Carter administration.



Solar School Project, Myeka High, KwaZulu/Natal, South Africa. Myeka High's solar array and satellite dish bring distance learning into the classrooms.

By 1997, his modest operation had established eleven self-sustaining solar energy projects in eleven countries across Asia, Africa, and South America.

"We're seeking to accelerate commercial market acceptance of solar-generated electricity in developing countries through showcase projects, technology transfer, technical assistance, youth training, grass-roots financing mechanisms, and multilateral development bank support," Williams says.

It's a hefty agenda and, so far, SELF has been surprisingly effective in effecting it. In western China, SELF has brought sun power to 1,000 households in fourteen villages, created the Gansu PV Company to manufacture small-scale photovoltaic systems as a joint venture with SELF, and established the Gansu Solar Electric Light Fund to extend credit to villagers to buy the systems. In Sri Lanka, it helped a national development agency start a division to sell photovoltaic systems at prices that villagers can afford but that still will enable the agency to sustain itself. In Tanzania, SELF has worked with the Masai people—a widely-scattered group of herders—to help the tribe acquire solar-powered telephones and FM radios to share information about land speculators threatening to drive them off their ancestral lands. In a poor area of black South Africa, SELF installed a photovoltaic system in a school and used the energy to power computers and connect the school to the Internet.

But, over time, SELF began to evolve more elaborate project structures. In one joint venture in India, SELF formed a for-profit subsidiary with local partners. Through India's Renewable Energy Development Agency, the venture tapped World Bank funds set aside specifically for photovoltaic installations. In part, the company used the money to finance rural co-op's bulk purchase of solar-energy systems for its members, install the systems, train local technicians, then repaid the World Bank's loan from funds the company collected from the co-ops.

In 1997, SELF gathered assets it developed through for-profit partnerships in India, Sri Lanka, and Vietnam to form SELCO, the Solar Electric Light Company. SELCO seeds new for-profit partnerships and helps to manage existing ones; SELF continues as a nonprofit entity developing new demonstration projects. For its part in establishing the partnerships, SELF received a 20% equity stake in SELCO.

"There's a lot of capital flowing into the developing world, but it's building Nike factories and five-star hotels," says Williams, now SELCO's president. "How does that benefit the ordinary person in these countries? We've shown that there are markets in these countries where capital can earn a profit by building an essential infrastructure for ordinary people."

That infrastructure can do far more than run lights and radios in homes, says Robert Freling, SELF's executive director. "This technology is so versatile that it can be used to improve the quality of rural life in areas of health, education, micro-enterprise, and communication. SELF's challenge now is to develop programs and projects that demonstrate—through examples like the South African school—that this technology can be used in a holistic, environmentally benign way for rural development."

But he's really talking about two technologies. One is photovoltaic hardware. The other is the method that SELF has pioneered to finance and grow an infrastructure of decentralized, renewable energy. That method now stands ready and waiting for other new energy sources about to be born.

Solar Electric Light Fund • 1775 K St. NW #595 • Washington, DC 20006
202-234-7265 • www.self.org • solarelectric@self.org



Bennett Daviss contributes to several national and international magazines including *Discover*, *New Scientist*, and *Air & Space*.

Earth Day! Not Again?

Remy Chevalier

Editor's Note: In this Invited Opinion, we are pleased to present the heartfelt frustrations of a dedicated environmental activist with the traditional green movement. Though we may not agree with his every sentiment, we applaud Remy Chevalier's wake-up call to mainstream environmentalists to consider fairly the prospects of cold fusion and new energy. They should examine the scientific evidence, ignore the negative propaganda, and—to steal from a phrase from the sixties—give infinite energy a chance! —EFM

If former U.S. Senator Gaylord Nelson was the father of Earth Day, Denis Hayes was its engine. Thirty years ago they managed to get thirty million Americans out into the streets to protest the abject corporate impact on the environment. In 1990, these same corporations became Earth Day sponsors, lavishing this country in a sea of greenwash. It had come around full circle. Little progress had been made. For every Band-aid solution, ten other disasters loomed ahead with no quick fix. The Reagan administration took its toll. Denis went from being director of the Solar Energy Research Institute in Denver, back then stripped of all funding, to being president of the Bullitt Foundation in Seattle, another one of those very wealthy green tax-shelter organizations with top-heavy objectives and nebulous results.

I met Denis in 1989 when I booked him as a speaker for an Eco-Saloon at Wetlands, a New York nightclub fronting direct-action groups. We talked about the role the military could play in the environmental movement. Pentagon procurement eventually became a major supporter of Eco-Expo. Then in 1996, I contacted Denis again about work I was doing trying to bring attention to "new energy" research. I did call him an ill-advised icon for not taking these possibilities into consideration. But I was stunned by his reaction. He lumped "new energy" research with "hopes to power humankind with Swedish Stones and alchemy," adding he had "no tolerance for conscious fraud."

We have since lost touch. Earth Day degenerated into Keep America Beautiful. Where I live, in the Connecticut suburbs, the roads are still littered with Budweiser cans and now Snapple bottles. Nothing has changed very much. Yes, there is a photovoltaic industry, but it's a blip on the radar screen of the oil companies that own them. General Motors just recalled most of its few electric cars because of some convenient fire hazard. American rivers might be a bit cleaner than they were twenty years ago, but in Europe and elsewhere, they are dead or dying, like the Danube. Earth, from space, shows the deserts gaining ground and the ice caps melting. Humanity has decided immediate survival is more important than long-term ozone concerns. The Rio and Kyoto agreements were not worth the paper they were printed on.

So what can another Earth Day do? In the spirit of *50 Things You Can Do To Save The Planet*, published back in 1990, and its three dozens variation on the theme, Denis has written a new little booklet: *The Official Earth Day Guide To Planet Repair*. Island Press publishes academic environmental books. Their titles rarely reach super-bookstores. I can't understand why this book which was meant as another how-to for Earth Day activities, had a publishing date of March 15, only a month before the event! Shouldn't this book have been out over the summer, giving a chance for people to plan things out?

The aspiration this year is that the Internet will make it a much more vibrant and immediate event. But in the wake of the WTO Seattle riots, I'm sure Earth Day organizers were not ready to really rock the boat. They lavished the distinction of celebrity spokesperson Leonardo DiCaprio, a guy who, let's face it, is a great actor, but a spoiled brat. He has spent the last two years partying on the rave beach scene of Thailand under the pretext of shooting a movie. Interestingly enough, the crew was blamed for trashing its location. Whether that accusation is true or not,

knowing Union workers, I wouldn't put it past them. Hollywood has two green think tanks, Earth Communications Office and the Environmental Media Association. Both are great at producing public service announcements, but have a hard time cleaning up their own house.

Leo went on TV with Denis this winter with a never-got-up-this-early haze, pontificating about his love for the environment. He followed this stellar performance with some incoherent rant in a *Rolling Stone* interview about having to choose between eating beef or tuna. What's going on here? What has all this degenerated into? We are faced with biosphere problems of cataclysmic proportion. Europe has just experienced the worst storm ever recorded, which destroyed entire forests, including ten thousand trees in Versailles. An oil spill worse than the Exxon-Valdez on France's Atlantic coast three months ago was totally ignored by American media. A flood in Mozambique is right now killing hundreds of thousands of people. Same in Bangladesh and dozens of other places around the world where water levels are rising at an alarming rate. Venice's monuments are flooded six months a year.

Yes, Al Gore wrote a book called *Earth in the Balance*, from which

. . .there's been no effort on the part of any of these green mega-groups. . . to make the slightest effort to investigate the research regularly reported by periodicals like *Infinite Energy* magazine or *New Energy News*.

he's laid low ever since. A few years ago he was caught on camera explaining to a woman how eating beef was bad for you and then had to apologize to cattle ranchers. Did we expect Earth Day 2000 to be a "Gore For President" political rally? Wouldn't you be afraid this might backfire? People in this country are sick and tired of celebrity endorsements. Tired of holier than thou attitudes on the part of icons who keep falling flat on their face in tabloid scandals? Nothing is sacred anymore, and my stomach starts to turn when I see those whose art I respect, like R.E.M. or U2, fall prey to mindless politicians wearing blue suits and red ties.

I've been an environmental activist since I had an epiphany at sixteen in 1969 looking out over the ocean sky one night on a Florida beach. I was overwhelmed by the knowledge that the planet was alive, a real live organism. The Gaia Hypothesis had not been invented yet. For hours I stood there embracing the vastness of space as I could feel the earth cry out for help. To quote the bad guy in the movie "The Matrix": "man is a virus." Well, maybe I wouldn't go that far. So I became torn between a need to curb humanity's lifestyle and my gut response to my planet's perceived cries for help. This feeling later gave rise to sentiments of ecofascism on the part of those who never had these experiences of unification with the elements. Back in my youth when every religion swarmed in my brain, I finally understood how some fundamentalist Christians could see the environment as a satanic concern. Only Lucifer would care about Earth's fate since this was his only home. Christ and those he saves will just magically restore the Kingdom. This explained a lot of inertia. A lot of Americans were just living in that hope, at the expense of harsh realities. Six billion mouths to feed, bodies to warm, 1000 million smog-spewing vehicles on wildlife-destroying asphalt roads! A Crumb cartoon nightmare.

I started looking for solutions, "real" solutions. What could do away with the internal combustion engine and provide us with clean, unlimited energy? I worked on behalf of solar power for two decades until it became apparent to me it would never make a dent in the power structure. I started discovering the work of renegade inventors who had ideas about tapping energy from the primordial field, "CHI," but who didn't have any more financial resources to make it happen than I did. The conspiracy of energy invention

suppression is all too real. So when a man like Denis Hayes, whom I looked up to when I was a teenager back in the 1970s, described my aspirations at organizing a Manhattan Project-styled program to bring all these inventors together a "conscious fraud," it really, "really" hurt me! How can a man who was such a supporter of solar energy be so opposed to something that might finally solve all our problems, twenty years later? Like the oil companies invested in oil, he was now invested in solar. That's why.

His *Guide To Planet Repair* is just that, "repair," like the patch kit you buy for bicycle tires. The Bullitt Foundation is worth \$100 million dollars. The Energy Foundation, on whose board Denis also sits, spends \$20 million a year promoting and sponsoring "renewable" energy projects. Yet, there's been no effort on the part of any of these green mega-groups, another being the \$1.7 billion Packard Foundation, to make the slightest effort to investigate the research regularly reported by periodicals like *Infinite Energy* magazine or *New Energy News*. They are trying to solve old paradigm problems with old paradigm technology. It's doomed to fail. Earth Day has become a corporate fest, when in fact it should be a return to certain Pagan rites of respect for the astral body that gave us life. A lot of astronauts have come back down to Earth with the same conviction. How can we ever hope to bring Earth back in the balance if we don't tune into what it is trying to tell us?

When self-appointed saviors of our environment refuse to even look at work done by those who propose radical departures from conventional thinking, then these people become part of the problem. Are Denis Hayes, Leo DiCaprio, and Earth Day part of the problem? I think they mean well and try as hard as they can with the tools of comprehension at their disposal. But I also think it's time they made way for people who have a lot more forward vision, and who can now let in those who for years think they have a better answer, but were never given a chance to make their case known. It's the same old story all over again. The military for years has been playing with classified technologies deemed too dangerous to let loose on the population. These same technologies used to build mysterious weapons are the same technologies that could save the world. If you are a civilian inventor trying to implement these ideas in a peacetime manner, you are ignored by those whose role should be to give you a public forum.

So it was Earth Day again. More helium balloons. More sweeping the hemp legalization issue under the rug. More buy electric cars that don't exist because Detroit will never mass-produce them. And come April 23, it's back to business as usual, until there isn't one old-growth tree left standing in Oregon, until the Mediterranean is as dead as Long Island Sound, and a multitude of other calamities befall us. Why? Because \$300 billion is spent every year on national security instead of international security, and because we've erected a force field so high around our borders, America has forgotten it is part of the same globe as everyone else.

Remy Chevalier is former editor of *Electrifying Times* magazine. He is the Director of the Environmental Library Fund, 25 Newtown Turnpike, Weston, CT 06883.

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Renewable Energy News

Peter Glück*

At the end of World War II, we learned Einstein's magic formula $E=mc^2$ and we knew for sure that the power which destroyed Hiroshima and Nagasaki, if tamed and made peaceful, would solve all the problems of energy for all people, forever. And we waited. I decided to become a physicist, to continue Einstein's work, and to help build power plants, ships, cars, homes, and public buildings working with clean uranium and not with stinking oil.

However, due to a strange combination of my IQ, EQ, CQ, plus my LQ [www.acronymfinder.com] will show you that the first three are my intelligence, emotional, and creativity quotients, while LQ stands for "luck quotient," perhaps the lowest of all, if we consider six years of war, forty of communism, and ten of a difficult transition], which gave me karma, I had to change plans. I was not allowed to become a physicist because of my bad "social origin," that is, my parents had not been paupers. The highest position accessible for me was "chemical engineer" and I have dedicated forty-one years to this challenging and interesting, always dynamic, sometimes dangerous profession.

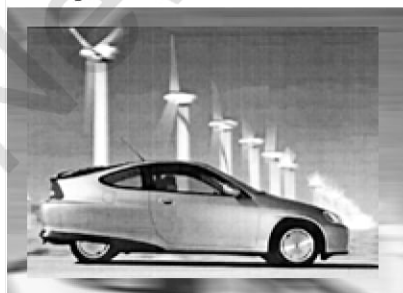
Quite early I discovered my passion for research and I learned how to manufacture tons or thousand of tons of a product initially obtained in a small beaker or a pilot plant. This is scale-up and due to my life experience I have a, hopefully benign, obsession with that. When reporting about renewable energy, my first criterion is again scale-up. Scale-up goes in many directions in space and time and includes intensification and multiplication of plants.

Regarding energy, we had been disappointed (we = our generation) by the slow scale-up of atomic energy by fission, but we were convinced fusion would go much better and faster.

In the meantime, Romania has built many thermal power plants working with low-quality coal and a bunch of hydropower plants. I could watch directly the effects on three beautiful rivers of my country: Bistrita, Olt, Arges, and. . . believe me, hydropower is not a 100% renewable source of energy!

Rivers cannot be killed, but they can become very ill. When I had almost finished this report, I came upon an article in *Village Voice*, March 29-April 2000, entitled "Dam Shame," by Karen Cook. It is about the world's greatest hydropower plant to be built on the Yangtze river in China, considered by many ecologists a future Chernobyl of hydropower. Perhaps this is too pessimistic, because the control of flooding also has to be taken into account. Anyway, a dam on a river is something like an aneurysm for a blood vessel.

A very interesting possibility to make hydropower more ecologically friendly is using the elastic reaction of water in addition to its potential energy. This system was elaborated and tested successfully by the International Molecular Reactor Power Company, Inc. of Stroud, Canada. The company's president and inventor, Nicholas Pataceano, has developed super fast jet valves—the critical element of the new source. Considering scale-up, the optimal dimension for the Pataceano system is that corresponding to 300 to 400 kW. The development work continues with insufficient funding.



Hhybrid vehicle, Honda2000 website

We learned that Einstein's formula is not magical enough in the case of fission-based nuclear plants, while hot fusion developed so slowly that even after two generations there is no practical use.

Then came cold fusion, with great

expectations and great fights, too young yet to be evaluated, but let's go back to our renewables.

I will deliver only brand new information, therefore the question is: what happened in 2000? The greatest event of this incipient year is hydrogen from common algae. The original paper where it was published is: Melis Anastasios *et al.* "Sustained Photobiological Hydrogen Gas Production Upon Reversible Inactivation of Oxygen Evolution in Green Algae *Chlamydomonas Reinhardtii*," *Plant Physiology*, 122, 2000, 127-135. A fine piece of science in any case, and a bright trick: the algae change their metabolism if lacking the essential element sulfur (as sulfates) in the growth medium and switch to a mechanism based on consuming all the available oxygen and then generating hydrogen in anaerobic conditions. This can be continued for some four days, and, after that the system has to return to the normal photosynthesis. All the scientific news agencies and the journal *Science* have hailed this work as a breakthrough in the hydrogen branch of the renewable energy field. A relevant title is "Power From Pond Scum" (*Science*, 287, 5458, March 3, 2000, 1580). If we look through our scale-up lens, it is difficult to share this enthusiasm. Scale-up goes through all the three elements of what can be called the Useful Trinity: Science, Technology, and Business, and this method belongs, for the time being, to the first entity. The new candidate of renewable energy source has just attained a productivity of 3 ml hydrogen per liter of algae culture per hour, a "thimbleful." The authors claim this could be easily increased ten times. A gallon of gasoline can be replaced by 1.2 kg of hydrogen or 13,400 liters (of gas) and this comes in ten hours from a pond having a volume of 4.5 cubic meters, with increased activity. Hydrogen gas has to be collected, separated from oxygen, and stored. Not an easy task. The pond or bioreactor or whatever it might be, has to be controlled, the algae have to be alternatively fed and starved with/of sulfur, they need plenty of sun. . . a plethora of scaling-up problems to be solved. Not exactly a technological feast. I do not expect to be alive when "gas from algae" fuels cars. However, renewable energy has strong supporters, continuous funding, and a very good perpetually and indestructibly optimistic press, so such minor details are neglected with elegance and style.

Perhaps the recent progress in the field of fuel cells is more significant technologically. This is described in the paper: "Direct Oxidation of Hydrocarbons in Solid-oxide Fuel Cells," *Nature*, 404, 265, 2000; authors S. Park, J.M. Voss, and R.J. Gorte are from the University of Pennsylvania, Philadelphia. Their cell is able to work with ordinary fuels, such as methane and other hydrocarbons. It does not rely exclusively on hydrogen which could be the fuel of the future, but for the time being it is "difficult to produce and dangerous to store, making fuel cells expensive and of limited appeal" (according to *Nature*). Fuel cells are ecologically friendlier than burners. They generate directly electrical energy and release only water and carbon dioxide. Successful scale-up, including exponential multiplication is possible; for what else could we wish? Obviously a lot of regular, incremental progress takes place in all the domains of renewable energy—you can easily find relevant information at the websites mentioned in my previous paper (*Infinite Energy*, No. 30, p. 42).



Solar dish generator, Warren Gretz

Glück, Renewables continues on page 69

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Report on the Second International Conference on Fuel Cell Vehicles Held on February 23-24, 2000, London

Soo Seddon

A somewhat higher profile affair than last year, due mostly to more media interest. I found myself rubbing notebooks with representatives from CNN, Reuters, the Energy correspondent of the *Financial Times* and tripping over snaking cables liberally laid by a team from the Tokyo Broadcasting Corporation.

Japanese TV was there to film the scheduled progress report by Dr. Kawatsu of Toyota but, following an apparently eleventh hour substitution, the opening presentation was, in fact, given on behalf of DaimlerChrysler.

Tokyo Broadcasting's enthusiasm for recording the rival manufacturer's presentation, however, was seemingly undiminished.

Michael Koss, DaimlerChrysler's General Projects Manager, began by describing the very political nature of German R&D. He told how his company had been ordered by the Federal government to assess the efficacy of various fuel cell fuels and report their findings back, as soon as possible, to the Department of Energy. The Germans, he said, were looking for serious results.

So, DaimlerChrysler is currently investigating their Big 3—liquid hydrogen, methanol, and liquid natural gas—with a mandate to come up with a clear strategy for the utilization of just one of these three options or, certainly, a maximum of two.

He went on to discuss the general pro's and con's of each of these fuels, eventually concluding that whilst methanol and natural gas, both of which could use existing fuel delivery infrastructures with minimal adaptation, were financially attractive in the short term, liquid hydrogen with its greater potential for primary energy, plus the zero emission benefit, had to be a very serious contender in the fuel stakes.

He expressed concern, however, that the levels of CO₂ currently released during traditional H₂ manufacturing processes were a potential stumbling block to large-scale production. That said, he gave the distinct impression that DaimlerChrysler wouldn't be lightly crossing hydrogen off their ultimate "maximum of two" list in the immediate future.

His presentation prompted some interesting questions from the audience, possibly because he was clearly attempting to give very little away. In fact, there was a much more positive feel to the entire conference this year. Things, I suspect, are at last warming up on the commercial front.

A delegate from IMRA Europe noted that DaimlerChrysler had summarily rejected dimethyl ethane (DME) on the grounds that CO₂ reduction was nowhere near sought-after levels using these fuels and asked for an opinion on why rival manufacturer, Volkswagen, had recently rejected methanol-based systems.

Koss admitted that he too had been stunned by this announcement. Volkswagen had initially been firm proponents of methanol as a fuel but had suddenly, and inexplicably, switched exclusively to hydrogen research based, he guessed, on the results of some long-term viability projections.

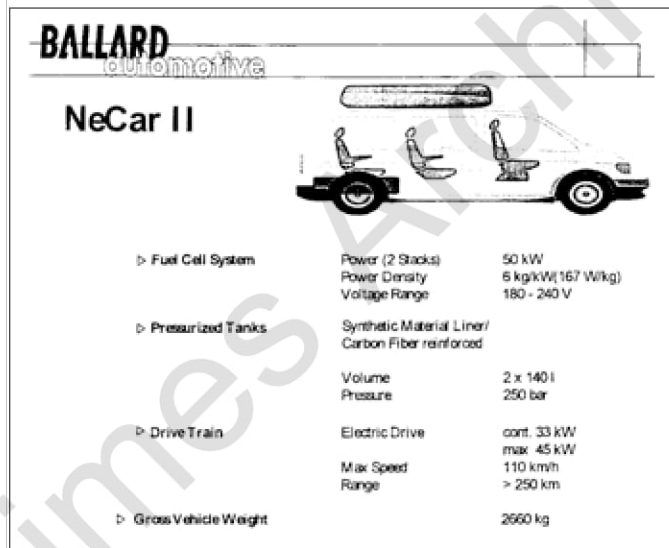
He expressed his great surprise at Volkswagen's public pronouncement, however. So amazed was he, in fact, that he declared himself to have been (literally) "almost falling backwards over."

When asked somewhat wryly by a delegate from Peugeot if, in rejecting methanol, Volkswagen might start a trend amongst German FC (fuel cell) vehicle manufacturers, Koss would only say that H₂ production costs were still quite high and, while his company is currently researching more cost effective methods of acquiring bulk liquid hydrogen, he could comment no further on the subject which was currently "commercially very sensitive."

DaimlerChrysler was the only vehicle manufacturer gracing the conference podium this year and, perhaps predictably, soon after Koss finished speaking the Japanese film crew made their excuses and left—thankfully taking their earthworm farm of cables with them.

Ballard spokesman, European Marketing Manager Andre Martin, who gave a short presentation at last year's conference, reported his company's "tremendous progress" in reducing the overall weight and volume of fuel cells and fuel cell systems.

To illustrate this, he cited the various stages of the Necar Project to date (NeCar II shown below) showing how the gradual reduction in stack size, with no apparent loss in power density,



had resulted in vastly improved vehicle design. He stressed the importance of an internationally integrated refuelling system. There was, after all, little point in building vehicles that were restricted to one area of the country, or to just one country.

Ballard's stated aims were that fuel cell vehicles must start to compete with ICEs as soon as possible and this was achievable only by the application of mass manufacturing technology and the use of low-cost materials.

In the long-term, he envisaged many cost-saving benefits, not the least of which was that no moving engine parts meant vehicle maintenance costs would be almost negligible.

Martin noted, however, that not all vehicle manufacturers were yet willing to make a total commitment towards fuel cell technology. Volkswagen, he stated, is still working on an alternative internal combustion engine.

He believed generally that the R&D path was not yet clear and that it would be two to three years before the true direction of research became defined.

The year 2004 was Ballard's target for FC vehicles to become practical and volume production was expected from then onwards. He hinted at "exciting future prospects" now that it was possible to transfer electrical power by wire, and went on to say that over the coming year his company will focus on further increasing the power density of fuel cells in order to provide real commercial features for fuel cell electric propulsion systems.

The DaimlerChrysler and Ballard presentations were by far the more interesting ones and prompted many diverse audience questions, ranging from the social and political benefits to be gained by wresting power from unscrupulous "oil barons," to the dubious morality of introducing pollution to third world countries by stimulating the demand for more vehicles.

Fuel Cell Vehicles continues on page 70

Patterns of Success in Research Involving Low-Energy Nuclear Reactions

Mitchell R. Swartz*



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Abstract

A metanalysis of the peer-reviewed literature (not conferences) of low-energy nuclear reactions/cold fusion (LENR/CF) research was undertaken. In 1989, LENR/CF phenomena generally had a success ratio (res+/res-) of ~ 0.5 -1. This ratio increased to greater than 3.6 (by 1997) and now may approach ~ 9 . Although the likelihood of success in 1989 of controlling LENR/CF reactions was less than 30%, today it appears to be generally closer to $\sim 90\%$.

These analyses indicate that there is greater reproducibility of the LENR/CF phenomena, due both to better understanding of what is required to produce the phenomena and controlling of false positives and negatives.

Introduction

LENR/CF phenomena have been confirmed in experiments involving replications and expansion of the technology in laboratories in over thirty countries.¹⁻³ This considerable effort by the scientific community has resulted in a massive, developing volume of cold fusion papers, many published in peer-reviewed journals and others presented at technical conferences or published in technical publications.

Much discussion has been made about the quantity and quality of the scientific LENR/CF literature. A number of reports have been cited by a good, but limited, set of selected papers⁴ in which skeptics of this new science attempt to incorrectly claim the publication rate is decreasing or that there is an alleged failure to reproduce the LENR/CF effects.

Since the March 1989 announcement of the discovery of low-energy nuclear reactions (LENR), also known as cold fusion (CF), we have reported on developments in the field and some of the associated nuclear science and engineering technologies.

Previously, we reviewed this matter and demonstrated that the facts are more complicated based on our metanalysis of the LENR/CF literature which comes closer to the entire LENR/CF data base.¹⁻³ Briefly, several LENR/CF data bases exist. The skeptics use a data base which excludes conferences. A reasonable alternative is to include all papers because of the generalized difficulty of publishing papers in the LENR/CF field. Figure 1 shows the publication rate (papers/year) during 1989 onwards in the LENR/CF field.

Shown are the publication rate (papers/year) from 1989 onwards. The upper thick curve is the cumulation of all LENR/CF papers including conferences worldwide, and the thick curve below it

presents papers from the United States. The annual publication rate both worldwide and in the United States are shown by the thinner curves in Figure 1.

The annual rate of published articles in LENR/CF continues unabated in this field (mean 227 papers/year; range 100-400). Thus, it was shown that it is reasonable to consider all research in a field before concluding that R&D has ceased.

Thereafter, we analyzed aspects of the steady increase in scientific paper output in the field and have analyzed the LENR/CF efforts, selected for scientific papers (experimental and theoretical), sorted by country and state within the United States.³ These numbers are larger than those generally claimed based upon the more limited Britz bibliography⁴ which excludes LENR/CF conference proceedings and reportedly some of the journals abstracted in *Chemistry Abstracts* and *Physics Abstracts*.

In the following discussion, the numbers indicate the LENR/CF experimental paper output in 1995-1996 (first number in parenthesis) and the cumulative scientific LENR/CF paper output from 1989 through 1996, which includes theoretical and peripheral papers (second number). As we reported, the main countries contributing to LENR/CF scientific effort are Japan (110,343), United States (86,930), Russia (52,198), India (28,71), Italy (13,108), France (11,41), and China (10,81). Although the United States leads with more than 900 articles published, Japan with a third of this amount

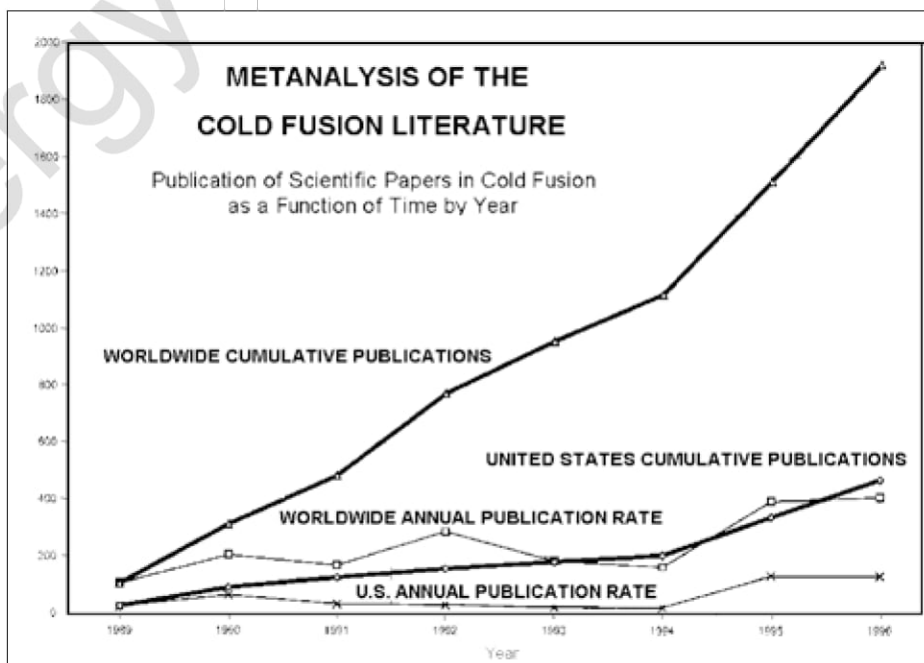


Figure 1. Metanalysis of the LENR/CF literature. Four curves showing the cumulative number of publications and the annual publication rate. The upper thick curve is the cumulative sum of all LENR/CF papers—including conferences—worldwide. The lower thick curve below represents those papers arising from the United States. The annual publication rates, both worldwide and in the U.S., are shown by the thinner curves below.

has a greater number of experimental articles published (110 vs. 86 in the 1995-1996 time period). Most of the recent experimental efforts in the United States have occurred in California (30,108), Texas (17,61), Massachusetts (6,28), New Mexico (6,33), Pennsylvania (4,9), Florida (3,6), Minnesota (3,7), and Utah (3,64).

We now continue our metanalysis of the literature by focusing on the changes in the patterns of failure of LENR/CF as determined from the selective data base (excluding conferences).

Experimental

The selective data base⁴ maintained by Dr. Dieter Britz (Chemistry Department, Aarhus University) was used for the cold fusion papers publication statistics. The papers, which Britz independently regarded as positive results (res+, that is, directly supporting cold fusion) or negative results (res-, that is, not confirming the LENR/CF phenomena), were catalogued by year. This data was collected and then the ratio of res+ to res- was determined. In addition, the percentage of all results described in the papers which were positive was derived ($\text{res+}/(\text{res+ and res-})$) defined as $\%(\text{res+}/(\text{res+ \& -}))$. Those papers for which the authors did not conclude either a positive or negative result were excluded from this study.

Results

Figure 2 shows the plots of the ratio of res+ to res- and the percentage of all res (results) papers which were positive by year from 1989 onwards.

Interpretation and Conclusions

Figure 2 shows that in the first year of LENR/CF research the ratio of successful to unsuccessful results (defined as res+/res-) first was only ~0.5-1. Thus, the negative papers dominated and the patterns of failure were such that investigators failed to reproduce the LENR/CF phenomena. However, it can be seen that increasing through 1993-1994 the ratio became about ~2.6-3.6. The positive result papers dominated the negative papers by a factor of about 3. In fact, by 1997-

1998, the ratio approached 9, but further investigation is underway in this region because not all of the papers may have been counted.

The increase in the ratio of successful LENR/CF research we believe is due to better understanding of what is required to produce the phenomena. The initial peaks probably included understanding of isotopic loading as being a 'key' to producing the phenomena successfully.⁵⁻⁸ The later increase may be due to further understanding and control of false positives and negatives as well.⁹⁻¹²

Figure 2 also shows the likelihood of success which is defined as $\% \text{res+}/(\text{res+ \& -})$, that is, the ratio of res+ papers to all res papers considered (res+ and res-). The likelihood of success the first year of LENR/CF reactions was less than 30%, but increased progressively until 1998, where it may be closer to 90%.

The first conclusion is that both of these analyses indicate that there is greater reproducibility of the LENR/CF phenomena. An alternative interpretation is that those experiments which are negative are not being reported to the same degree as they were in 1989 and shortly thereafter. However, it seems unlikely, as the skeptics of the LENR/CF phenomena have neither disappeared nor lost interest in stating their case.

Another result is that this study corroborates the previous reports¹⁻³ which heralded that some interpretations of the LENR/CF data set based on subsets of the published LENR/CF papers—and those papers citing that data—may have errors, including the claim that there is an "exponential decrease" in the number of LENR/CF papers or that LENR/CF has not been reproduced.

There are limitations, in that the data used was that of a simple metanalysis and not an inventory of each sample tested by all the researchers.

Furthermore, the selected data base used was by Britz, and did not include conference papers and selected journals which tend to have more positive results. The results of this paper (and the data on Figure 2) may, therefore, be a lower limit to the developing success in this field with the success ratio and the likelihood of success actually being higher.

Summary

In summary, despite an unprecedented reluctance on the part of many peer-reviewed journals to publish cold fusion articles, the appearance of peer-reviewed publications in the field continues, even though it has decreased from the 1989-1990 peak. One important result of this analysis is that it appears that although the unrelenting skepticism in the United States has severely decreased the scientific publication rate in the LENR/CF field, the worldwide publication rate has taken up the slack and continues unabated, possibly increased in 1995-1996 compared to previous years.

The likelihood of success [$\% \text{res+}/(\text{res+ \& -})$] during the first two years of LENR/CF reactions was less than 30%, but increased progressively until 1998, when it may have been closer to 90%. These analyses indicate that there

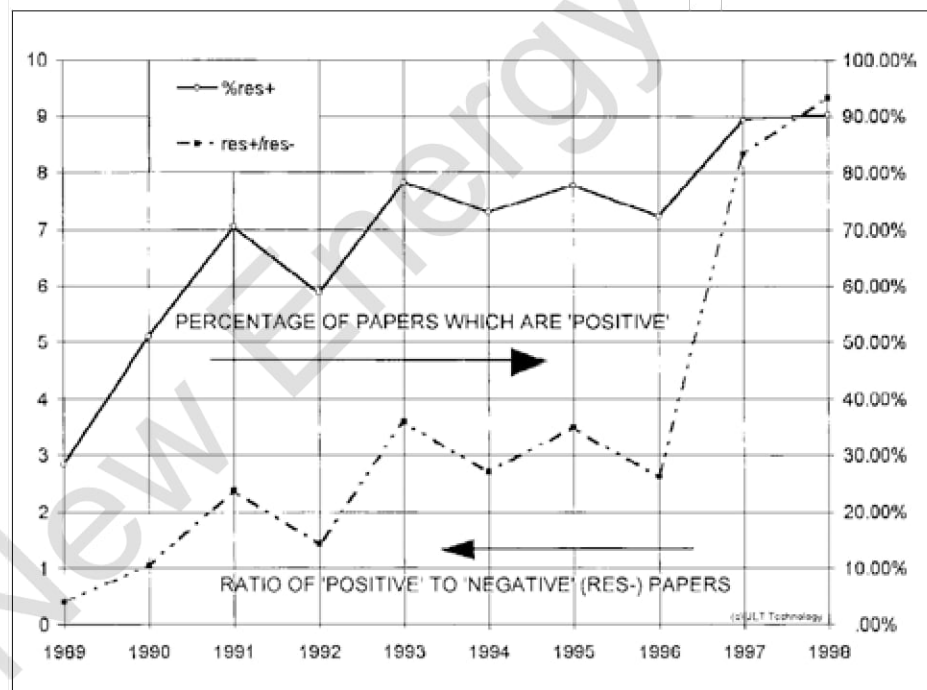


Figure 2. Metanalysis of pattern of successful to unsuccessful experiments in cold fusion by year as determined by the authors of the papers. Shown are the ratio of success (res+ to res-) and the percentage of all results papers which were positive by year, from 1989 onwards, with curves which link the relevant sets of data. Data after Dieter Britz.⁴

is greater reproducibility of the LENR/CF phenomena.

We have shown that in the first year of LENR/CF research the ratio of successful results (defined as res+/res-) first was only ~0.5-1. However, it can be seen that increasing through 1993-1994 the ratio became about ~2.6-3.6, and by 1997, the ratio of success in this field has approached 9. The increase in the ratio of successful LENR/CF research, we believe, is due to better understanding of what is required to produce the phenomena and controlling false positives and negatives as well.

Acknowledgments

The author gratefully thanks Dieter Britz and Hal Fox for some of the raw data, and Gayle Verner, Andrew Pawl, Eugene Mallove, Barbara DelloRusso, Ed Wall, and others on Vortex-1 for their comments.

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About the Author

Dr. Mitchell R. Swartz (Sc.D., MIT, 1984; M.D., Harvard University, 1978; EE, MS, and BSEE, MIT, 1971) has contributed broadly to the fusion and energy literature on issues including

quality-control in calorimetric analyses, proton transfer reactions at electrodes required for fuel loading, and phonon-material interactions. His current research investigations at JET Energy Technology, Inc. include engineering of, and material development for, low temperature fusion systems, nuclear transmutation, break-even studies, and the more efficient use of computed systems for the integration of power generating systems.



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An Interview with Francesco Scaramuzzi

This interview was conducted by Douglas Siu-Kwong Lee when he was a senior in high school in 1997. He conducted interviews via e-mail with both Dr. Francesco Scaramuzzi and Dr. Edmund Storms for a term paper entitled, "Let's Open Up To Cold Fusion." Dr. Scaramuzzi is the Organizer for ICCF-8, so this is a timely contribution.

Douglas Siu-Kwong Lee: Describe your educational background.

Francesco Scaramuzzi: I have got the Italian equivalent of a Ph.D. in Physics, and I have been doing research continuously in the last forty years.

Lee: What is cold fusion?

Scaramuzzi: In experiments of electrolysis with palladium cathodes in heavy water, Fleischmann and Pons found that heat was produced, in such an amount that could not be explained in terms of chemical reactions. Thus, they suggested that it could be nuclear energy due to the fusion of two deuterium nuclei, taking place within the palladium crystal. Thus, cold nuclear fusion at room temperature is compared with about 100 million degrees of traditional "hot" fusion.

Lee: When did these experiments take place?

Scaramuzzi: Fleischmann and Pons did this in 1989.

Lee: How close are we to figuring it out?

Scaramuzzi: Research is going on mainly in Japan, in USA, in Italy, and in Russia. Progress is very slow, and it could take still years in order to "figure out," as you say, what happens in these experiments.

Lee: What problems do you foresee in achieving it?

Scaramuzzi: The main problem that I foresee at present is the lack of reproducibility in the experiments, which prevents a rapid development of the research and also is responsible for the skepticism surrounding this field. I have the feeling that this problem is being addressed correctly.

Lee: How useful can cold fusion be in today's society?

Scaramuzzi: About usefulness one must be very careful. The scientific problem is, in my opinion, very interesting, but speaking of practical applications is premature. But, if one tries to be optimistic and foresees a positive development, then the striking feature is that it seems to be clean nuclear energy, which is a dream of everybody.

Lee: Are there any negative side effects of its use?

Scaramuzzi: History teaches us that almost every discovery can be used in the interest of mankind or against it. Thus, it is hard to answer this question. Undoubtedly, the absence of relevant nuclear radiations is a positive feature.

Lee: Does the good outweigh the bad?

Scaramuzzi: Definitely. The prospect of clean nuclear power is worth the risk of it being abused.

Lee: Do you have any personal experience with cold fusion?

Scaramuzzi: I have been involved in cold fusion research since the beginning, and I am still active.

A Determination of the Nature and Velocity of Gravitation

Reginald A. Fessenden*

Submitted by Richard A. Ford of Champaign, Illinois⁺

Reprinted from *Science*, Volume 12, Number 307, November 16, 1900

The present note is to be taken as a supplement to my previous paper on "The Nature of the Electric and Magnetic Quantities and of the Density and Elasticity of the Ether."¹ A fuller development of the theory of gravitation advanced by the writer is in course of preparation. This will, however, be delayed for some time, possibly for several years, as it is desired to investigate certain phenomena rather more accurately than has hitherto been done, and at present the writer is occupied with pressing work in another line.

It has seemed advisable, however, to publish this determination of the velocity of gravitation at the present time, and without waiting to complete the fuller treatment, for the reason that, as will be seen later, the value obtained clears up a number of perplexing optical problems, and removes a number of obstacles which have hitherto stood in the way of the development of that branch of physics.

On account of the fact that the writer's papers on these subjects have unavoidably been published in somewhat scattered form, it is considered best to give a brief resumé of the work which forms the basis of the method by which this velocity is deduced.

In 1893 the writer perceived that Fourier's "Dimensions" could be developed into a very powerful agent of research, and one which should bear a relation to the usual methods similar to that which Qualitative chemistry bears to Quantitative. It was for this reason that the name "Qualitative Mathematics" was given to this new branch.

As its name signifies, it is used, not for the exact determination of numerical quantities, but for the prediction and classification of phenomena.²

It was first shown³ that the nature of electricity and magnetism was, at that time, indeterminate, as all the electric and magnetic phenomena which we were able to completely express dynamically could be comprised in three qualitative equations, whilst we had four unknown quantities.

Then, by several methods, Williams' result, that either specific inductive capacity (k), or permeability (μ), must be a density, the other term being a compliancy, was confirmed.

It was then further deduced that the one which is a compliancy must decrease with the first power, whilst the one which is a density must decrease with the second power, of the corresponding intensities, *i.e.*, if F be the electric potential difference per unit length, and H be the magnetic potential difference per unit length, then if μ be a compliancy, on increasing H , μ will decrease by an amount depending upon the first power of H , and on increasing F , k will decrease by an amount depending upon the second power of F . Also, in this latter case, the diminution of k must depend inversely upon the coefficient of volume elasticity.

On the other hand, if it is k which is the compliancy, these relations will be interchanged.

It was at once noticed that several of the empirical formulae expressing the relation between H and μ gave a diminution depending upon the first power of H . A somewhat elaborate investigation was then undertaken, extending over the greater part of a year, and the fact was definitely established that the diminution did depend upon the first power of H accurately, the maximum amount of deviation from that called for being less than one-fourth of one percent, which was about the limit of experimental accuracy.

This, of itself, would have sufficed to have settled the point, but in addition the other relation, which should exist if the theory were correct, *i.e.*, that the specific inductive capacity, k , should vary with the inverse second power of the slope of electric potential, and as the coefficient of volume elasticity, was also discovered. This was found to be the complete expression of Kerr's electrostatic phenomenon.

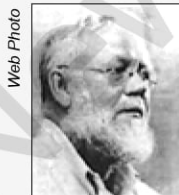
A prism of glass, 1 cm thick and 1 cm wide, stretched with a force of 30×10^6 dynes gave a change of density of nearly 3×10^{-5} . The change in the thickness of the glass was approximately 1.5×10^{-5} . The change in velocity of the light which passed transversely through the glass, was approximately $.7 \times 10^{-5}$.

It was thus found that the actual mechanically produced change in density of the glass was sufficient to account for the observed change of velocity, though the agreement was not so close as it might have been, possibly owing to experimental difficulties.

Historical Information on Reginald A. Fessenden

*In 1876, when he was only ten, Fessenden watched Alexander Graham Bell demonstrate the telephone in his lab in Brantford, Ontario, Canada. From his home in Fergus, Ontario and through his travels to Bermuda for a teaching position, Fessenden never forgot his dream of transmitting words without wires. After attaining a position in the laboratory of Thomas Alva Edison, Fessenden began his studies in electricity and transmission. He later worked for Westinghouse as well. In 1893, Professor Fessenden moved to Pittsburgh, Pennsylvania, and later served as head of the electrical engineering department at Western University (now the University of Pittsburgh). While there, Fessenden read of the radio experiments that Guglielmo Marconi was conducting in England and began experimenting himself. He devised the theory of "continuous wave," a means to superimpose sound onto a radio wave and transmit this signal to a receiver where the radio wave would be removed, leaving the listener with the original sound. It took six years for Fessenden to refine his invention, but on Christmas Eve 1906, from Brant Rock, Massachusetts, he made the first radio broadcast in history. Radio operators on ships in the Atlantic were shocked to hear a human voice emitting from the equipment they used to receive Morse code. Fessenden is credited with over 500 other inventions, including the Fathometer (or depth finder).

⁺Richard A. Ford wrote: "Your reprinted article on the speed of gravity made me remember another article from the year 1900 (while ether concepts were still in vogue). It is interesting to compare the two conclusions as far as speed of gravity. Fessenden was a well-known Canadian-American physicist who is generally credited with broadcasting (first) both speech and music."



Web Photo

From the observed change in velocity when placed in a strong electrostatic field, whose value was approximately determined by its sparking distance, it was calculated that the value of the $F^2/8\pi k$ stress required to produce the same change of velocity as had been produced mechanically was nearly 25×10^5 dynes. The value of the purely mechanically applied stress, as given above, was 30×10^6 . The close agreement is probably accidental, as the experimental error was considerably greater than the small difference observed. It is intended to repeat these experiments under conditions permitting of a much higher degree of accuracy.

The results obtained are, however, sufficient to show that Kerr's effect can be accounted for by purely mechanical stresses, electrically produced and resulting in a change of density.

Now it was pointed out above, that whichever of the medium coefficients, k or μ , varies as the square of the corresponding intensity, that one must be a density. Since, therefore, it has already been shown by Kerr that the change in velocity, and hence, as my experiments prove, the change in density,⁴ is proportional to the square of the electric intensity, it follows that k is a density.

It still remained to be shown that Kerr's effect depended upon the volume elasticity. This was done by testing different glasses and noting that, the compensating pieces being made from the glass under test, the same force was always required to compensate, independent of the material tested.

We see, therefore, that the results deduced from the experiments on the relation between H and μ are completely confirmed by the results obtained on investigating the relations between F and k .

A number of additional pieces of corroborative evidence were also given, *i.e.*:

1. The relation between the magnetic constant a and the elasticity.
2. The relation between this constant and elastic strain.
3. The relation between this constant and permanent strain.
4. The relation between this constant and hysteresis.
5. The relation between k and the density of substances.

Several phenomena were also predicted, *i.e.*:

- A. A change in the velocity of light, along a slope of electric potential.
- B. A relation between refractive index and piezo-electric effect in doubly refracting substances.

These have not yet been confirmed, but arrangements are being made to investigate the former.

This same result, originally obtained by qualitative mathematics, can also be obtained by Lagrangian methods. By considering the way in which permeability and specific inductive capacity are affected in the case of stressed iron, and in the case of Kerr's phenomenon, as influenced by the elasticity of the material, it can be shown that a change in μ involves the first power of the magnetic intensity and that a change of k involves the second power of the electric intensity. This proof will be given later. It, however, in reality, adds nothing to the proof already given, which in the opinion of the writer is of such a character that we may say that the nature of electricity and magnetism is now definitely and finally determined, though no doubt it may be years before the absolutely decisive nature of the proof is generally appreciated.

Next, it follows from the writer's experiments on the relation between H and μ that the presence of matter does not alter the elasticity of the ether by as much as one-fourth of one percent. Also, knowing now that k is a density, we are enabled to say that aberrational and other optical phenomena show that the density

of the ether is not appreciably altered by the presence of matter, otherwise the $(n^2 - 1)$ and similar formulae would not hold. From these facts we see that the actual volume of the atom, compared with the space occupied by it, must be quite small.

The diameter of the mercury atom I have shown to be $2.75 (\pm 0.2) \times 10^{-8}$, and in 1899 I showed that the actual cross section of the space actually occupied by the atom must be less than one four-hundredth of the space occupied by the atom to the exclusion of other atoms, and that the atoms "must have a configuration analogous, in its effects, to that of structures of thin platinum wire, suspended in oil."

Later, J.J. Thomson, from his beautiful and wonderfully ingenious work on electric discharges in gases, was able to show that the atom is made up of a number of smaller bodies, which he calls corpuscles.

On comparing the results of Thomson, Ewers, Kaufmann, Lenard, Lorenz, Wiechert, and Simon, we arrive at the conclusion that there are about 1,000 corpuscles in a hydrogen atom, and that the weight of a corpuscle is therefore about 1.5×10^{-27} gm.

Since, then, there are about 200,000 corpuscles in a mercury atom, and their cross section is less than 1/400 part of the cross section of the mercury atom, we find that the diameter of the corpuscle is certainly less than 2×10^{-11} cm.

From J.J. Thomson's formula for the electrically produced inertia of a charged sphere, we find, as was shown by Thomson (and independently by the writer), that if the diameter of the corpuscle is approximately 10^{-13} cms, the ionic charge which it carries will account for its full quantity of inertia.

So long as we knew nothing of the size of the corpuscle, and since there might be a thousand corpuscles in a hydrogen atom, and yet each corpuscle be about 3×10^{-9} cms in diameter, we were hardly justified in holding that inertia is an electric phenomenon. But when we take into consideration, in addition, the writer's proof that the diameter of the corpuscle must be less than 1/1000 the diameter of the atom, and that this is the *superior* limit in size, we have a reasonable basis for holding, as the writer has done,⁵ that the corpuscular charges are the cause of the inertia of matter.

Assuming this, we arrive at the result that the corpuscle is about $3/4 \times 10^{-13}$ cms in diameter. The ionic equivalent being about $4 (\pm 1) \times 10^{-10}$ e.s. units, we find for the electrostatic tension and pressure at the surface of the corpuscle, about 2×10^{32} dynes.

One of the theorems immediately deducible by Qualitative Mathematics is that, "Whenever the electric or magnetic forces act in the presence of matter, the resultant effect is made up of two terms, one expressing the result of the action on the matter, the other that of the action on the ether."

We have seen that the electric stresses produce a change of volume in matter, and hence we must have also an effect of the same quality in the ether. Such a change of density in the ether would produce a gravitational attraction, and we may now calculate what value the ether constants must have in order to produce the observed amount of gravity which is associated with the corpuscle.

Taking Boys' value for the gravitational attraction of two masses, each of 1 gram, and 1 cm apart, *i.e.*, 6.65×10^{-8} , we get for the gravitational energy of the corpuscle about 10^{-48} ergs.

From this and the electrostatic stress we can calculate the volume elasticity, and we find it to be about 10^{73} .

But I have previously shown that the density of the ether is about 0.66 and its rigidity about 6×10^{20} .

Hence we can calculate the value of the compressional or gravitational wave, and find it to be approximately 5×10^{36} cms per second.

It will be at once seen that this value agrees with our astro-

nomical facts, and that it does away with a great many optical difficulties. For in the first place it makes the compressional wave vanish, and in addition, which is of the greatest importance, it makes the amount of energy in the compressional wave infinitesimally small.⁶

We may summarize our conclusions as follows:

The ether itself is a composite body, having a structure whose elastic properties are analogous to rubber. This is shown by the low value of the rigidity as compared with the compressibility, and by the form of the equation expressing the relation between H and μ .

This would immediately suggest a vortex theory, even if the quality of the ionic charge, *i.e.*, M/T , were not called to the attention. If we take Fitzgerald's vortex theory, and develop it along the lines indicated by my theory we have the vortices analogous to what, in the case of India rubber, I have called the "skein material," and the fluid in which the vortices form, which, until someone suggests a better name, we will call etheron, taking the place of the "filling-in material."⁷ The compressibility of the etheron is very high, as we have seen, it being the thing which determines the velocity of the compressive wave. The vortex structure is what is concerned in transmitting the light waves and its modulus; the rigidity modulus is much smaller and of a different order, just as in the case of India rubber.

We do not need more than one vortex, "the umbilical cord of the universe," as one aspect of it suggested itself, stretching with its ends fixed on some free surface of the etheron and itself forming one inextricable tangle. The circulation being the same everywhere simplifies matters. The parting of the vortex anywhere means the destruction of all matter.

Such a medium, as Fitzgerald has shown, gives an ether which can transmit light. Following up this theory, we conclude that corpuscles are vortex singularities, and that it is the hydrodynamic head of their flow which gives the etheron density-variation round them. This change in density varies as the fourth power of the distance from the corpuscle. All the gravitational energy tends to that of compression, and if two corpuscles come together, their gravitational energy goes to increasing the compression energy of the ether. They do not come together because their approach brings into play forces which depend upon the energy of the vortices themselves. The fact that there is but one vortex, and consequently the circulation is the same everywhere, gives the atoms definite sizes and the corpuscles the same quantity of electricity, *i.e.*, the ionic charge.

A group of so many thousands of these corpuscles makes up the atom. The inertia of the atom is due to the electromagnetic inductance of the corpuscular charge, and gravity is due to the change of density of the ether surrounding the corpuscles, produced by the electrostatic stress of the corpuscular charge. Mass and gravity thus bear a constant ratio.

The cohesive force of the atoms, as I have shown elsewhere,⁸ is due to the electrostatic attraction of the atoms for one another. Chemical force, as has been shown by Davy, Berzelius, Helmholtz, Ostwald, and other workers, is due to the same cause.

It may here be noted that the idea of the ionic charge as an ever-present element of the atom is an interesting example of a theory, negatived absolutely, apparently, by fundamental principles, and yet developing in spite of its apparent incompatibility with facts in many other directions, with such success as to finally obtain a firm footing, although the arguments against it have never been answered.

The fact that the ionic charge is the agent in chemical action has been shown by the physicists just mentioned above. The presence of charged ions in electrolytes had also been firmly established, and J.J. Thomson had suggested that conduction in

metals also took place through a breaking up of molecular groups, as in the case of electrolytes. But when in 1890 and 1891⁹ I introduced the theory that the ionic charge is attached to the atom, *not only when it is concerned in chemical actions or formed part of a molecule, but in every case and always*, and is the cause of a number of physical phenomena, such as cohesion, rigidity, etc., a number of objections were made; that charges could not exist in the interior of a conductor; that the atoms of metals must be conducting, and so could not have equal charges of electricity; and others, as for example, the well founded criticism of Ostwald (under date September 16, 1891): "The electrostatic theory of cohesion is new to me, . . . but for electrolytes there is the question to be answered, why stuffs like alcohol, etc., do not conduct? whilst according to *your* theory, *all* elements have electric charges."

These objections could not be met then, and have not been met up to the present time, in spite of the fact that this new concept (of the ionic charge being a fundamental part of the atom, apart from its chemical functions) has proved a most fertile one, and has been considerably developed by the originator and by later workers, Richartz, Chattock, Lorentz, Larmor, and others. Nor will these objections ever be met until we know the nature of metallic conduction.

This is one of the great outstanding problems. It has long been known that there is a relation between electric and heat conductivity. The writer has shown that there is a connection between the velocity of sound (and hence the elasticity and density) and the electric conductivity of wires. J.J. Thomson, as mentioned above, suggested that the current was carried by the electrolysis of molecular groupings, and his later work renders it probable that it is by means of the corpuscles. It is possible that the atoms of a metal are really dissociated and the negatively charged corpuscles are in a state similar to that of the ions of a solution, *i.e.*, the metallic atom is not a fixed combination of certain corpuscles, but is constantly changing in composition, the negative corpuscles being, as it were, in solution in the metal, and changing about freely.

Such an hypothesis would account for the relation between the velocity of sound and the electric conductivity. For the cohesion of the atoms would be due to these negative corpuscles acting, as the mortar between bricks, to bind together the positive groupings, and hence the greater the number of free corpuscles the greater the elasticity and the greater the conductivity, the conductivity being simply the number of free corpuscles per cubic centimeter. The greater the number of corpuscles in the positive groupings *i.e.*, the greater the molecular mass, the less the conductivity.

In presenting this summary I am aware, of course, that much of it is in need of further experimental evidence, and I hope, in time, to supply at least a part of this. It is considered, however, that the scheme here presented has a weight apart from its experimental foundation, in that it is a whole and consistent theory by which for the first time all physical phenomena are reduced to the simplest possible elements.

References and Notes

1. Fessenden, R.A. 1900. *Phys. Rev.*, January; and also of the earlier papers: 1891-2, on "The Laws and Nature of Cohesion."
2. Such a branch of mathematics is absolutely necessary to supplement the work done by the other methods. For the latter can tell us nothing of the *nature* of the quantity involved. Their very greatest strength is their greatest weakness. The fact that a certain function, which gives us the state of things at the end of an organ pipe, also gives us the way a

Gravitation continues on page 69

Contrasting Behavior: Pd Black in Contact with Pd Metal vs. 316 Stainless Steel

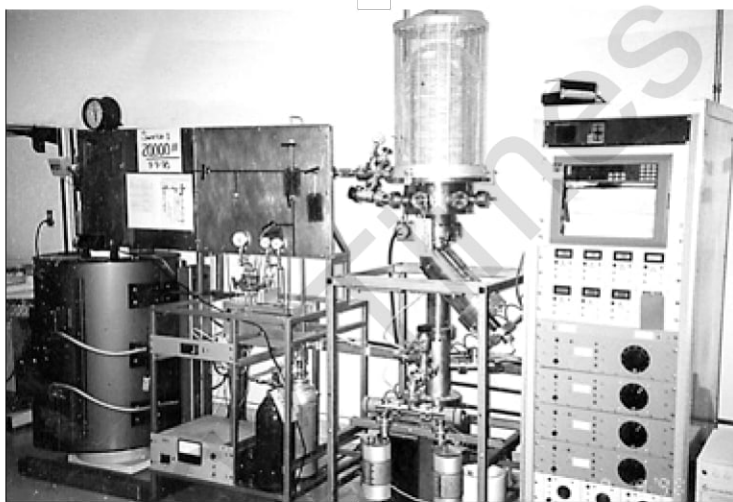
G.L. Schmidt* and T.A. Chubb**

The notable success record of DS-cathodes¹ in achieving Fleischmann-Pons heat release in electrochemical loading experiments focuses attention on the behavior of Pd black in contact with Pd metal while in the presence of deuterium atoms. The Arata-Zhang "double structure" DS-cathode consists of a Pd bottle-like container filled with Pd black. The bottle is hermetically sealed by an electron-beam welded cap with the powder encapsulated under clean high vacuum conditions. When operated as a cathode in an electrochemical cell in which D₂O is electrolyzed, D atoms are deposited on the outside surface of the DS-cathode, diffuse through the walls, make contact with the Pd powder, and recombine on the Pd surfaces to produce very clean D₂ gas within the enclosed volume. During prolonged electrolysis, the gas pressure has been observed to build up to pressures exceeding 600 atmospheres. Tests carried out at both Osaka University and SRI have shown that after three or more weeks of electrolysis, DS-cathodes loaded with deuterium produce radiationless excess heat and generate a helium nuclear product. However, it is not known why the DS-cathode produces heat more successfully than other types of Pd cathodes, and it is not known with certainty how and where the heat is generated.

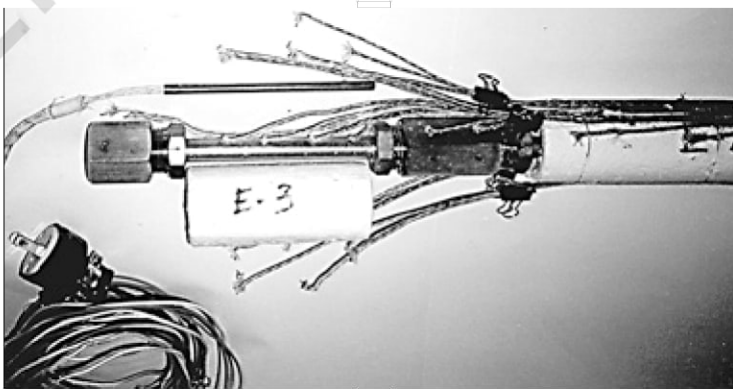
We report here an observation that may have a bearing on the success of the DS-cathode. First, however, we describe the results of recent excess heat studies at SRI and Osaka University. In October 1999 at the Western Regional Meeting of the American Chemical Society, Mike McKubre reported on four studies carried out at SRI.² He said that SRI had carried out over 150,000 hours of calorimetry. He stated, "The evidence, in my view, for the appearance of an anomalous unaccounted excess heat in the deuterium-palladium system is essentially overwhelming. There is something there. It's larger by more than one order of magnitude, in some cases by more than two orders of magnitude, than the sum total of all possible chemical reactions." He then described three electrochemical loading studies, plus a gas loading study using D₂

gas in contact with a Pd-on-carbon catalyst. The first two electrochemical studies showed excess heat calorimetric measurements quantitatively correlated with the presence of helium-4 gas in the electrolysis outgases. The amounts of helium observed were found to match an expectation value based on 24 MeV of energy release per helium atom ending up in the gas, assuming that some of the gas remained trapped in the Pd metal until the Pd metal was subjected to thermal and electrochemical cycling. The third study was the gas loading + catalyst study. It also showed a quantitative correlation between heat generation and helium generation at 24 MeV per helium atom. The fourth study was a study using Japanese-prepared DS cathodes, with one cathode used in a heavy water electrolysis cell and the other in a normal water cell. The cells were operated in series. The normal water cell showed no excess heat, while the heavy water cell showed about 8 W of excess heat, demonstrating similar behavior to that recorded by Arata and Zhang in Japan.

Arata and Zhang have continued their studies of heat generation with DS-cathodes in heavy water cells. They have now published data from more than eight runs.³ They always see excess heat using properly sealed DS-cathodes and heavy water. In one interesting recent study, they operated a DS-cathode in series with a solid Pd metal cathode of the same external geometry.⁴ They recorded excess heat with the DS-cathode, but saw no measurable heat with the solid metal cathode. In earlier experiments they showed the presence of strongly bound helium in Pd black recovered from previously operated DS-cathodes, and not in as-received Pd black.³ Heating of the powder to high temperature was required to expel the residual helium atoms. In a late 1999 study they succeeded in sampling the gas within a post-run DS-cathode. No heating was used. A strong helium-4 signal was observed. The gas sampling was achieved by puncturing a post-run DS-cathode bottle placed inside a special sealed sampling chamber.⁵ Although the Fleischmann-Pons effect has been confirmed in these



High pressure, high temperature test facility at the NMERI laboratory at the University of New Mexico. The high pressure manifold is behind a protective shield. Tags locate high pressure valves controlling gas flow into and out of the manifold. The bell jar and associated pumps exhaust the test manifold to $\sim 10^{-9}$ Torr prior to tests. Gas tanks below the high pressure manifold supply deuterium gas. Large vertical cylinder on left is a vertical tube furnace surrounding a pressurized test cell and associated calorimeter.



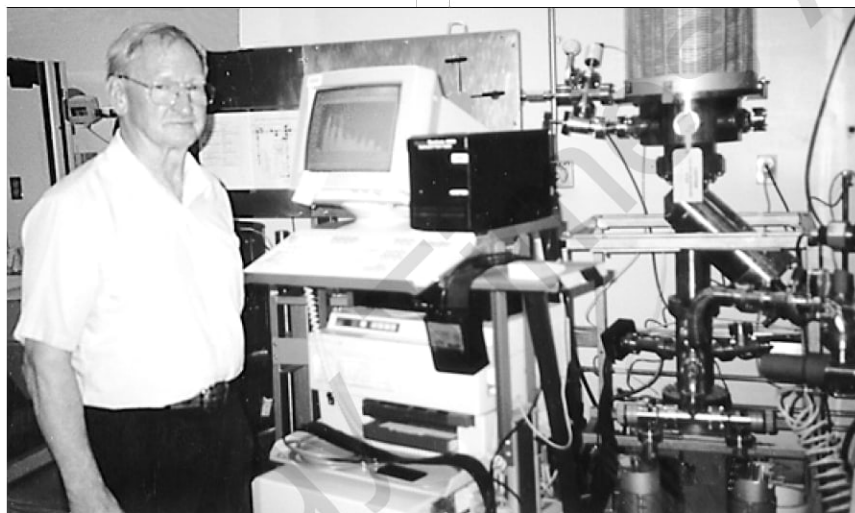
The piece of stainless steel pressure tubing above "E-3" is a test cell containing a sample of palladium black. Thermocouples are welded onto the test cell to measure heat flows along the cell. In a previous program, palladium powder samples were tested at 30000 psig and 300°C. The test cell was modified for the tests described in this paper.

studies and shown to be the radiationless release of nuclear energy by the $D^+ + D^+ \rightarrow {}^4\text{He}^{++} + 24 \text{ MeV}$ reaction, the conditions necessary for heat release have not been established. Does the heat release require the creation in a host metal of a D-chemical potential in excess of that which can be obtained in relatively simple chemical engineering reactors? The catalyst studies indicate that extreme conditions are not always required. Is this because the Pd-on-carbon catalyst provides a special environment for energy generation, *e.g.* lots of surface area, or maybe a Pd-on-carbon interface, or a multiplicity of small isolated Pd crystals? Other types of Pd catalyst have not worked. Studies of Pd black exposed to D_2 gas at a range of pressures up to 2000 bar and at elevated temperature up to 300°C have failed to produce excess heat at the 1 W level. What is so special about good Pd-on-carbon catalyst? All the other successful experiments have deposited deuterium onto Pd metal cathodes at a relatively high D-chemical potential. The DS-cathode studies have been at near-ambient temperature, and seem to work best at internal D_2 pressures on the order of 1000 bar, but some heat generation is observed prior to achievement of very high internal gas pressure. These values contrast with the one to five bar pressures and 175 to 200°C temperatures used in the catalyst work and correspond to considerably higher D-chemical potentials.

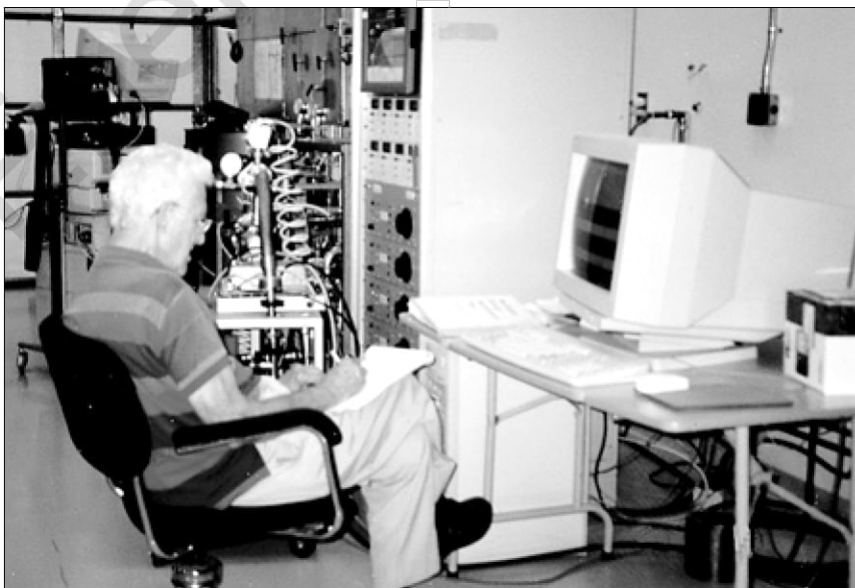
There is no general agreement as to the degree of thermodynamic equilibrium achieved in the DS-cathode experiments. Arata and Zhang believe that D_2 gas in contact with the Pd powder within the DS-cathode is not in equilibrium with the surface concentration of D on the Pd powder grains. The D atoms diffusing through the solid walls of the DS-cathode are viewed as making contact with the grains of Pd black and spreading by surface flow throughout the mass of Pd powder while also diffusing into internal structures within the grains. Independent of the powder, the DS-cathodes seem to lend themselves to a circulation of deuterium atoms, with atoms entering the Pd metal at portions of the surface having high current density and leaving the external surface at locations where the current density is lower. Does this D presumed atom flow result in heat generation in the solid metal? The observations

reported here discuss the effect of the combined presence of very clean Pd black, D_2 gas, and with the powder in contact with Pd metal at ambient temperature, as compared with the powder in contact with Type 316 stainless steel tubing under the same conditions. The test equipment had previously been used in a search for heat generation in Pd black at elevated temperature and pressure. The test assembly was reconfigured to see if it could be used in a search for heat from circulating D_2 gas in contact with Pd powder and/or clean metal. As-received Pd black was placed inside a stainless steel tube together with a piece of clean 200 mm long, 2 mm diameter Pd metal wire. The wire was surrounded with Pd powder. The test cell was then pumped down by a turbo pump and subsequently by an ion pump to about 10^{-8} Torr in an oil-free environment (10^{-9} Torr at the pump inlet). The Pd was then loaded with deuterium by sequentially introducing three aliquots of D_2 gas at 760 Torr. Absorption of the gas loaded the metal and powder into the beta phase at ambient temperature. The D_2 pressure then was raised to ~100 psig and the test material kept at this pressure for two days. Next, exploratory tests were carried out over a two-day period in which D_2 gas flowed through the mass of powder with the gas pressure kept in the 10 to 15 psig range. The cell

was evacuated a second time, back-filled with argon gas, and disassembled for examination. On disassembly in an oxygen-free glove box filled with ultra-pure argon, it was found that the Pd wire could not be easily removed from the stainless steel tubular test cell. When the wire was forcibly withdrawn from the test cell, the previously shiny wire was found to have an adhering dull coating. The grains contacting the Pd metal seemed to have become part of the metal, so that removal of the grains exposed bright metal crystals belonging to the wire. Microscopic examination showed that masses of powder had apparently grown from "nucleation" sites that had formed at the metal-powder interface. Dislodged powder appeared as metallic grains within the Pd black recovered from the test cell. The metallic grains sometimes showed die



Glen Schmidt measures the composition of residual gases in the test manifold using a portable quadrupole mass spectrometer.



Talbot Chubb observes operations during a high temperature test. Test parameters are shown on the computer screen using LabView.

***Pd Black continues
on page 63***

Testing Results of Ethanol at Crystal Energy, Inc.

Harry Taplin*

The state of California is going to remove MTBE from its reformulated gasoline because of concern over contamination of water supplies. MTBE came into use because of effective lobbying by the "very rich" petroleum industry.

The purpose of MTBE was to reduce ambient CO levels. It is an oxygenate that brings an additional 2% oxygen to the combustion process via the reformulated fuel. It is also supposed to raise octane performance slightly. We used ethanol in our testing, which is the accepted alternative to MTBE, without the octane boost.

What the public doesn't know is that the oxygen sensors on vehicles are calibrated around a point which creates CO, because the catalytic converter needs the energy contained in CO gases to keep hot and functioning. Precision oxygen sensors must be kept at precise temperatures to maintain calibration; the exhaust manifold of a gasoline engine, where they are installed, is not known for its temperature stability. Try to wrap your wits around this logic! (The reformulated gasoline is providing more oxygen, which is being countered by the oxygen sensor and the on-board computer to keep the oxygen lower in the engine exhaust to keep the catalytic converter hot by burning pollutants.)

We know that our fuel catalyst [see "Nuclear Augmented Combustion," *Infinite Energy*, No. 18, for a description of lithium catalyst] also improves the octane level because knock sensors used in testing at the Pittsburgh Applied Research Laboratory indicated reduced engine knock from moderately severe to just barely detectable with an 84 octane fuel on a new GM six-cylinder engine.

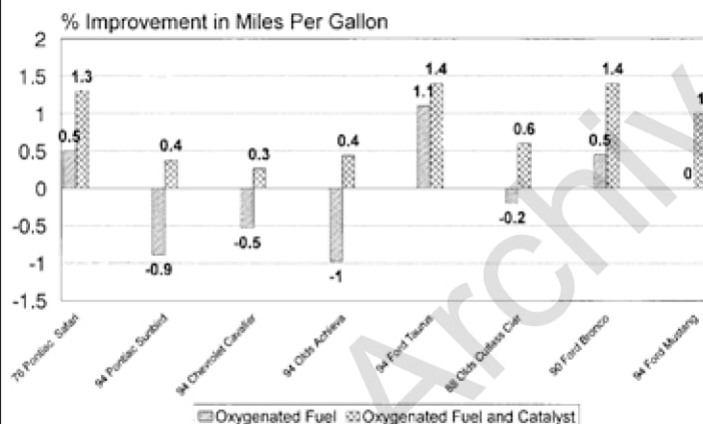
It is ironic indeed that in tests we performed for the SCAPCD, which were paid for by California taxpayers, a substantial improvement in CO reduction was demonstrated. Our test results showed that when using the lithium cold fusion catalyst, carbon monoxide and hydrocarbon emission reductions were greatly superior to MTBE results.

The testing also demonstrated 400% to 700% more oxygen at the tailpipe because of the conversion of nitrogen to oxygen by the reaction (see Circa 1919 discoveries by Ernest Rutherford). Test instrumentation and a California instrument manufacturer at an accredited automotive testing laboratory provided calibration for this series of tests.

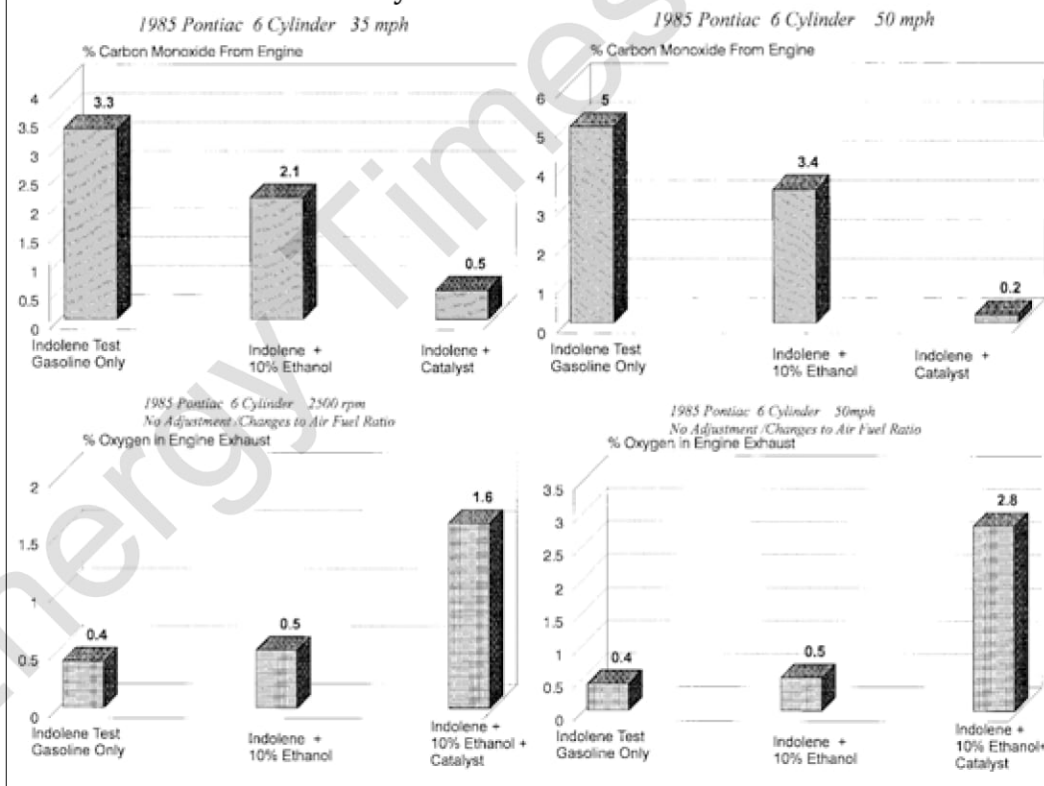
*Crystal Energy, Inc., 127 West Janss Road, Thousand Oaks, CA 91360, Phone: 805-495-1684, Email: hrtaplin@earthlink.net

MPG Changes

Vehicles Tested With Emissions Controls

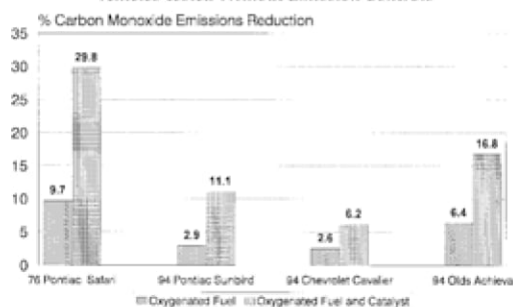


Dynamometer Tests



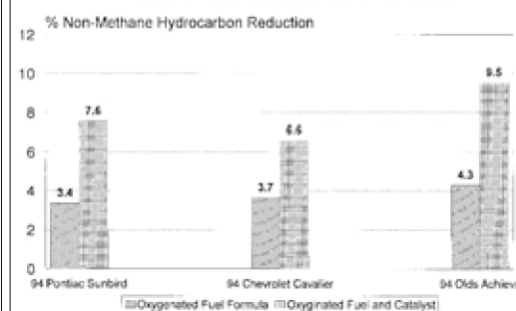
Carbon Monoxide Reduction

Vehicles Tested Without Emission Controls



Hydrocarbon Reduction

Vehicles Tested Without Emission Controls



The Nucleon Cluster Model and Thermal Neutron Fission

R.A. Brightsen*

Editor's Note: Infinite Energy has previously covered the multi-decade work of Ronald Brightsen and his Nucleon Cluster Model (NCM), which originated in the late 1940s:

Brightsen, R.A. 1995. "Application of the Nucleon Cluster Model to Experimental Results," *Infinite Energy*, 1, 3, 13.

Brightsen, R.A. 1995. "Nucleon Cluster Structures in Beta-Stable Nuclides," *Infinite Energy*, 1, 4, 55.

Brightsen, R.A. 1995/96. "Correspondence of the Nucleon Cluster Model with the Periodic Table of Elements," *Infinite Energy*, 1, 5/6, 73.

Bass, Robert W. 1996. "Experimental Evidence Favoring Brightsen's Nucleon Cluster Model," *Infinite Energy*, 2, 11, 78.

Brightsen's basic concept is that nuclei are composed of three kinds of nucleon clusters—NPN, PNP, and NP (where N is a neutron and P is a proton; note that the ordinary hydrogen nucleus, P, is a very special case, according to Brightsen!).

One may wonder why we are now publishing a paper by Brightsen which was written in late 1989 and put in revised form in March 1992. This paper deals primarily with explaining patterns in conventional thermal neutron fission. Brightsen attempts to show the NCM's application to what one might have thought was a topic on which "the book had been closed." As Brightsen states: "An immense

amount of experimental data has been accumulated on nuclear fission, certainly one of the most complex nuclear reactions observed in nuclear physics. Despite the data accumulation, a comprehensive theory or model which accounts for all the known data has not yet been developed."

The advent of low-energy nuclear transmutation phenomena, which has been observed in electrochemical systems by researchers such as Professor George Miley at the University of Illinois, and Doctors Mizuno and Ohmori at Hokkaido University, makes new models of nuclear structure of great interest. After all, these researchers appear to be seeing characteristic "cold fission" fragment distributions in their data. Could it be that Brightsen's Nucleon Cluster Model has some explanatory value in such experiments, as it appears to have for thermal neutron fission? We hope that the long-delayed publication of this paper will prompt some more mathematically inclined investigators to determine, once and for all, whether Brightsen's model of thermal neutron fission is "just" some kind of curve-fitting, or whether it perhaps has some deeper significance for nuclear theory and perhaps, as Brightsen believes, to cold fusion and new energy.—EFM

Abstract

Utilizing a new Nucleon Cluster Model (NCM) of the nucleus, the overall nature of which is summarized herein, the process of thermal neutron fission is described. Known experimental results are explained and quantified. Using the NCM, fission yield curves, the average number of prompt neutrons per fission ($\bar{\nu}$) and the emission of light charged particles (LCP) are all derived. The quantitative relationships presented provide an excellent fit to well-known data and systematically describe a framework for the fission process. The NCM suggests that fission takes place in four modes. This paper describes the light and heavy fragments for each fission mode, as well as their unique and systematic prompt neutron and light charged particle yields. Well-established experimental results for thermal neutron fission of U-235, as well as U-233, Pu-239, and Pu-241, are accurately reproduced for the first time since fission was discovered in 1939. These results provide support for the validity of the NCM.

Introduction

Thermal neutron fission of U-235, discovered in 1939 by Hahn and Strassman,¹ has been investigated intensively by physicists worldwide for five decades. An immense amount of experimental data has been accumulated on nuclear fission, certainly one of the most complex nuclear reactions observed in nuclear physics. Despite the data accumulation, a comprehensive theory or model which

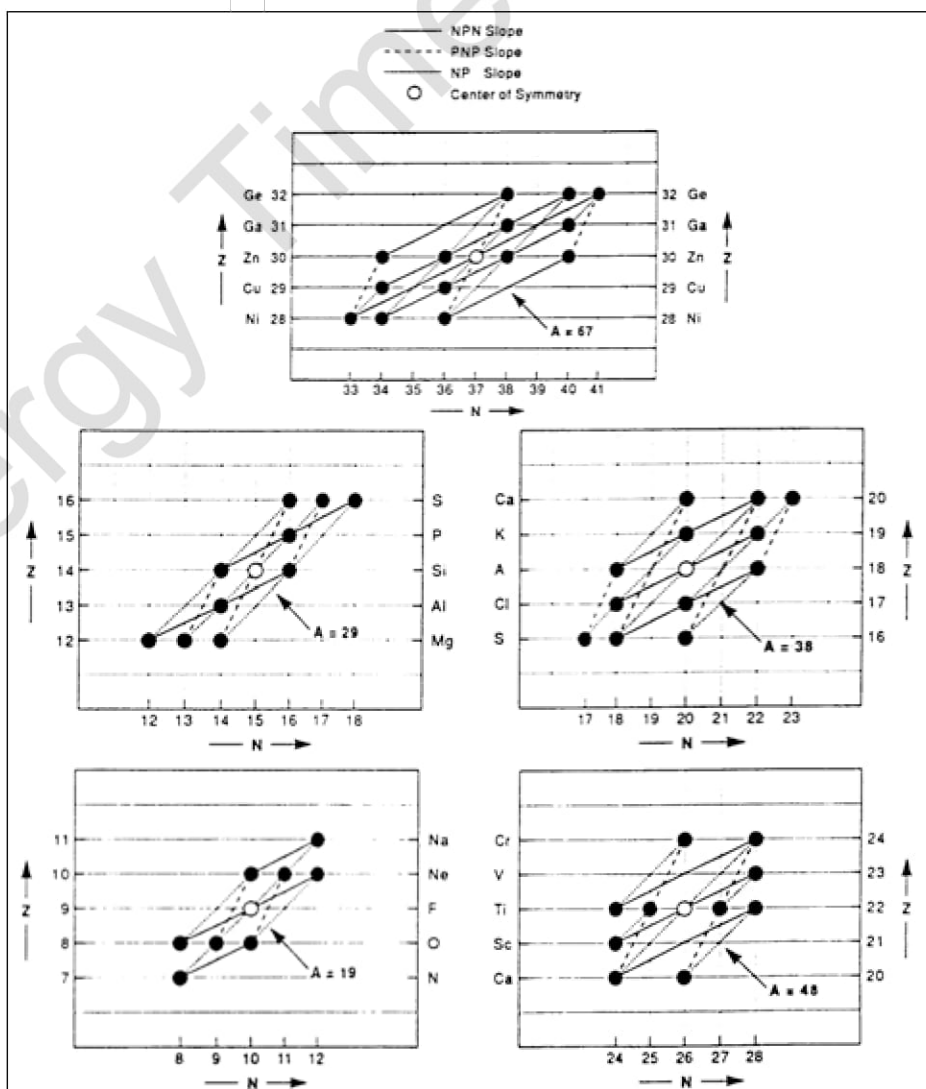


Figure 1. Mass-Charge Symmetry Systems. ○ ● Beta stable nuclides

accounts for all the known data has not yet been developed.

The research reported in this paper provides deterministic answers to four of the major unresolved questions regarding thermal neutron fission by utilizing the Nucleon Cluster Model (NCM), which is a new concept in nuclear clustering.² Specifically, these four questions are:

- 1) What accounts for the asymmetric mass distribution³ of the fission fragments in thermal neutron fission of U-233, U-235, Pu-239, and Pu-241?
- 2) What is the origin of the prompt neutrons that produce, in the aggregate, the experimentally observed average number of neutrons per fission ($\bar{\nu}$)?
- 3) What mass splits account for the variation in $\bar{\nu}$ from zero in some mass splits to as many as six in others?
- 4) What is the origin of the many light charged particles (especially He-4 and H-3) observed in fission?

Nucleon Cluster Model (NCM) and Nuclear Fission

The NCM resulted from a long and persistent search for beta-stability selection rules that would be applicable across the

entire mass range. In 1983 that research uncovered systematics that are valid from the lightest to the heaviest nuclides. The key was the discovery that all nuclides can be described by *three* nucleon clusters (NPN, PNP, and NP). Such cluster structures are in striking contrast with conventional nuclear physics, which describes all nuclides with only two parameters, N and Z (where $Z \equiv P$). Because of the revolutionary nature of this new nuclear model, it was decided to postpone publication until convincing evidence could be published in support of the NCM. Such evidence is presented in this paper. It explains, for the first time and in a remarkably accurate and deterministic manner, many of the observed experimental results of thermal neutron fission.

At the same time, it may be important to point out what the NCM does not yet explain. It does not deal with the "morphology" of the nuclides, *i.e.* it does not address the manner in which the clusters are arranged within each nucleus, nor does it deal with the shape or size of the clusters. We believe that these questions will ultimately be answered. At the present time, the NCM leads one to conclude that there are at least two significantly different binding energies involved in nuclear binding: the binding energies within and between the clusters.

A key observation leading to the development of the NCM was the recognition of heretofore unreported mass-charge symmetry systems among certain groups of beta-stable nuclides. Such symmetry becomes apparent in Z (proton) vs. N (neutron) plots, some of which are shown in Figure 1. It was immediately apparent that these centers of symmetry had a simple relationship among the mass numbers, *i.e.* $19 + 48 = 67$ and $29 + 38 = 67$, but no obvious relationship connecting the nuclear charge (Z) values. The three slopes (NPN, PNP, and NP) evident (see Figure 1) in these mass-charge symmetry systems imply that nuclei contain clusters consisting of NPN, PNP, and NP.

The probable connection of the NCM to fission was first noticed in two relationships: 1) $A = 67$ is very close to the beginning of the light "tail" in fission and 2) a pronounced fine structure peak in thermal neutron fission of U-235 occurs at $A = 134$ (2×67). Further research in the application of the NCM to the fission process resulted in the development of a "Periodic Table of Nuclides," a portion of which is shown in Figure 2. The horizontal scale of the independent variable A is folded into Periods of $\Delta A = 67$, and each Period is folded into two sub-groups. This latter folding, together with symmetry considerations, requires two junctions for each Period in the Periodic Table of Nuclides. In the fission fragment mass range shown in Figure 2, the following three junctions occur:

	Junction			Junction			Junction		
$A_{\text{light}} \rightarrow A_{\text{heavy}}$	82	84	86	115	117	119	149	151	153
$A_{\text{heavy}} \rightarrow A_{\text{light}}$	86	84	82	119	117	115	153	151	149
Σ_A	168	168	168	234	234	234	302	302	302

The vertical scale of the dependent variable Σ_3 represents the number of triplet clusters (NPN plus PNP) in each mass number A plotted. The numbers in the circles represent the number of doublet clusters (NP) in each of the plotted A values, all along the sloping lines connecting squares that are inscribed with the mass numbers. The regular periodicity of $\Delta A = 67$ is evident in Figure 2. It should be noted that this periodicity continues for mass numbers lighter than, as well as heavier than, those found in fission.

The periodicity of $\Delta A = 67$ is an inherent property of the NCM. The significance of $\Delta A = 67$ may eventually be revealed when the mathematical/theoretical bases of the NCM is developed. Note the frequent occurrence of this number in Figure 3, derived from Figure 2.

There is a striking correspondence between the branches of

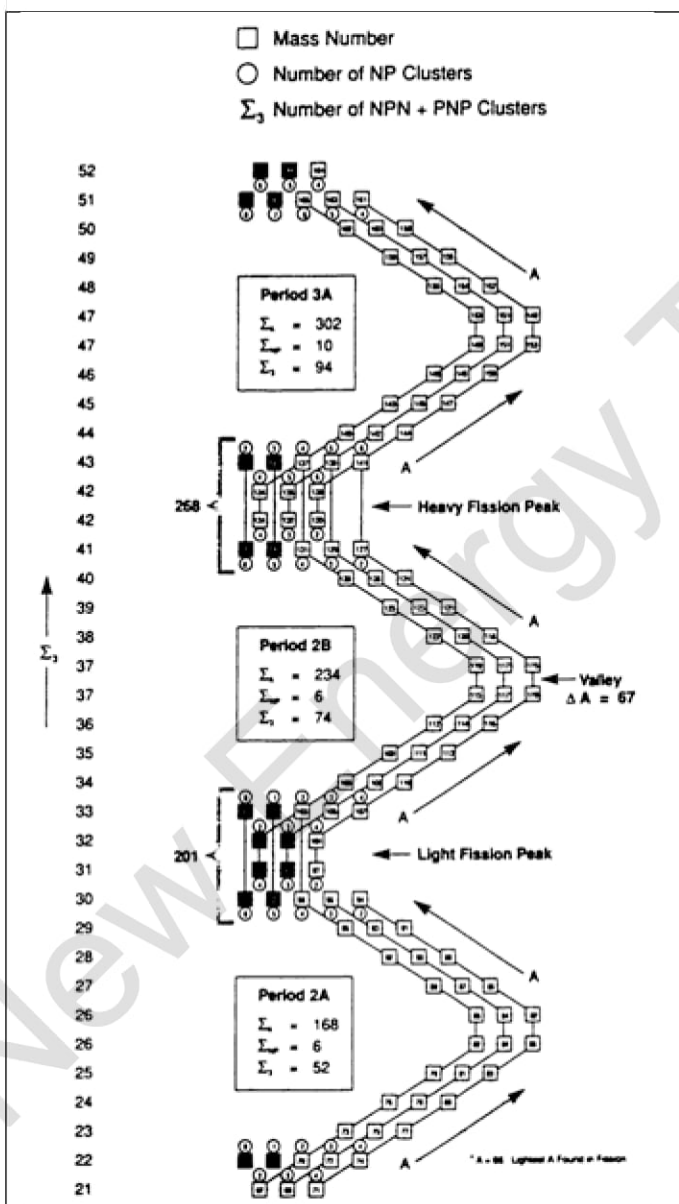


Figure 2. Periodic Table of Nuclides. Periods 2A, 2B, and 3A ($A = 66 - 169$). Figures are shown as provided by the author.

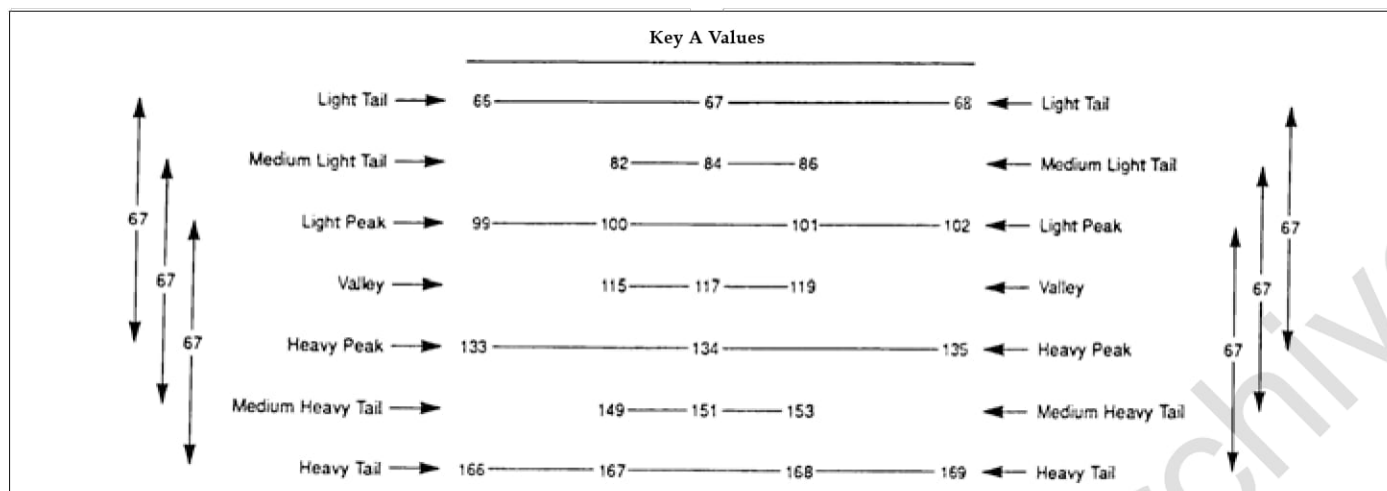


Figure 3. Thermal Neutron Fission of U-235. Fission-related Key A Values of the Nucleon Cluster Model.

the Periodic Table of Nuclides shown in Figure 2 and the five major features of fission yield curves for each of the four fissile nuclides (U-233, U-235, Pu-239, and Pu-241). The five features of those yield curves are: 1) light tail, 2) light peak, 3) valley, 4) heavy peak, and 5) heavy tail. This paper focuses principally on the correspondence between Figure 2 and one of the fissile nuclides, namely the experimental fission yield curve of U-235, shown in Figure 4. The correlation between those two systems, the NCM and an experimentally determined fission yield curve, applies to fission fragments ranging from A = 66 (known in fission) to A = 169 (not yet found in fission). An inspection of Figure 2 reveals the following:

- 1) The Junction in Period 2B corresponds to the "valley" in thermal neutron fission of U-235.
- 2) In thermal neutron fission of U-235, the junction of Period 2A and 2B corresponds to the "light peak," while the junction of Period 2B and 3A correlates with the "heavy peak," respectively.
- 3) Periods 2A and 3A represent the "tail" from the light peak and heavy peak, respectively.

The strong correlations between the characteristics of Figure 2 (summarized in Figure 3) and the experimental U-235 fission yield curve (Figure 4) precipitated a decision to conduct research on U-235 thermal neutron fission to test the validity of the NCM. This research has been supported in part by the Electric Power Research Institute.

The Four Thermal Neutron Fission Modes of U-235

It should be noted that when U-235 absorbs a thermal neutron, it becomes the fissioning compound nucleus, U-236. Fission occurs in four distinct modes, explained in further detail below, that are characterized by the type and number of particles emitted and by the mass range of the mode. Mode A releases one neutron, Mode B generates two neutrons, Mode C boils off six neutrons, and Mode D emits three neutrons as well as alpha particles and tritons.

The unique correspondence between the two systems—the Periodic Table of Nuclides and the experimental fission yield curve—is no coincidence.

Mode A: This fission mode is derived directly from the Periodic Table of Nuclides. Period 2A and 3A (Figure 2) sum to 235, suggesting that this mode systematically releases one neutron per fission. The light fragments of this mode extend from A = 66 (the lowest A value found in fission—excluding the very light charged

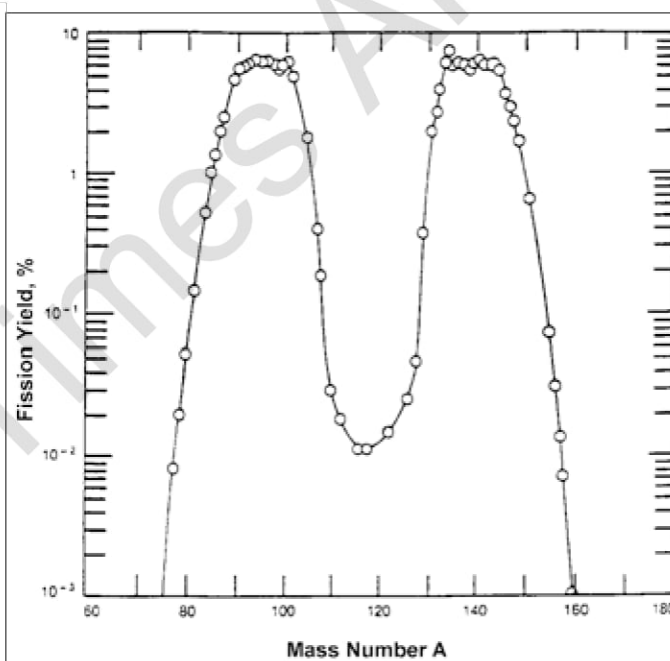


Figure 4. Thermal Neutron Fission of U-235.

particles) to A = 102, with corresponding initial heavy partners from A = 170 to A = 134. Each fission in Mode A emits one neutron, from the heavy fragment. This is consistent with experimental evidence, which indicates that more neutrons are produced by heavy fragments than by light fragments. Thus, subsequent to a single neutron emission from the heavy fragment, the mass of the light and heavy fragments sum to 235 as follows:

Light Fragment	66	102
Heavy Fragment	169	133
SUM	235	235

It should be noted that:

A = 66 is the beginning of the "light tail" in the fission yield curve, while A=169 is the end of the "heavy tail." (See Figure 4.) A = 102 is part of the light peak and A = 133 part of the heavy peak in the fission yield curve. (See Figure 4.) A = 66, 102, 133, and 169 are Key A values of the NCM. (See Figure 3.)

Mode B: This fission mode is also derived directly from the Periodic Table of Nuclides. The two sub-groups of Period 2B sum to 234 (Figure 2), suggesting a mode which systematically generates two neutrons per fission. The initial light fragments in Mode B begin at $A = 100$ and extend to $A = 120$, while the heavy initial fragments correspond to $A = 136$ through $A = 116$. Each fission in Mode B releases two neutrons, one from the heavy fragment and one from the light fragment. Thus, subsequent to neutron emission, the mass of the light and heavy fragments sum to 234 as follows:

Light Fragment	115	99
Heavy Fragment	<u>119</u>	<u>135</u>
SUM	234	234

It should be noted that:

$A = 115$ and 119 are at the approximate center of the "valley" in the fission yield curve. (See Figure 4.)

$A = 99$ and 135 are part of the light and heavy peaks in the fission yield curve. (See Figure 4.)

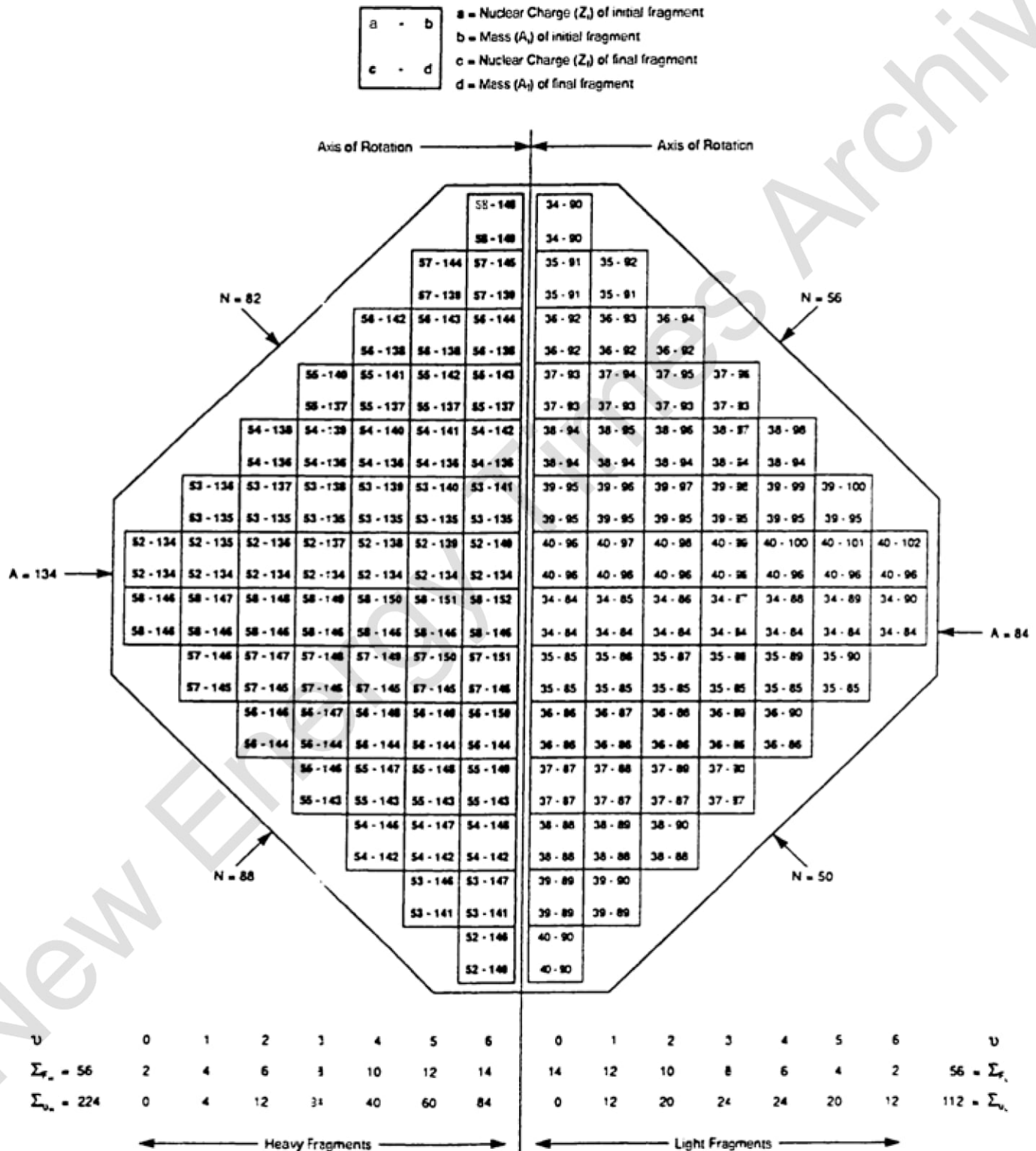


Figure 5. Thermal Neutron Fission of U-235. Fission Mode C.

A = 99, 115, 119, and 135 are Key A values of the NCM.
(See Figure 3.)

Clearly, no combination of Modes A and B could yield an experimentally known value of $2.416 \pm .008$ for $\bar{\nu}$, the average number of prompt neutrons per fission.⁴ Consequently, research was initiated using the NCM as a guide to develop one or more modes to:

- 1) explain the experimentally known fact that, in U-235 fission, ν has values of 0, 1, 2, 3, 4, 5, and 6 for various mass splits;
- 2) explain the influence of the "magic numbers,"⁵ N = 50 and N = 82;
- 3) explain the experimentally observed ratios of the light charged particles, especially the He-4 and H-3 ratio of 16.

Mode C: It has long been known that, in thermal neutron fission, the light side of the heavy peak fission yield is nearly a constant,⁶ and this persistent pattern has been attributed in part to the closed-neutron (N = 82) shell found in this mass region. This knowledge led to a postulate that systematic neutron emission might occur when the initial fragments contained 83, 84, 85, 86, 87,

and 88 neutrons, with no neutron emission when the initial fragment contained 82 neutrons. Such a model would certainly account for the experimentally observed variation of ν , while explaining the influence of N = 82. This "boil-off" model might also extend to N = 50. After considerable research, Fission Mode C was developed (Figure 5). This systematic but complex mode boils off six neutrons for each fission, with twice as many neutrons coming from heavy fragments as from light fragments. In this system, one fragment may release X neutrons (with X having values of 0 to 6) while the other fragment will release 6-X. It should be noted that the final heavy fragments in the upper left quadrant all contain 82 neutrons, while their light fragment counterparts contain 56 neutrons. Similarly, the final light fragments in the lower right quadrant all contain 50 neutrons, while their heavy counterparts contain 88 neutrons. Finally, it should be noted that the postulated Mode C indicates that the initial light fragments commence at A = 84 and terminate at A = 102, while the initial heavy fragments commence at A = 134 and terminate at A = 152. These A values are all key NCM A values. (See Figure 3.)

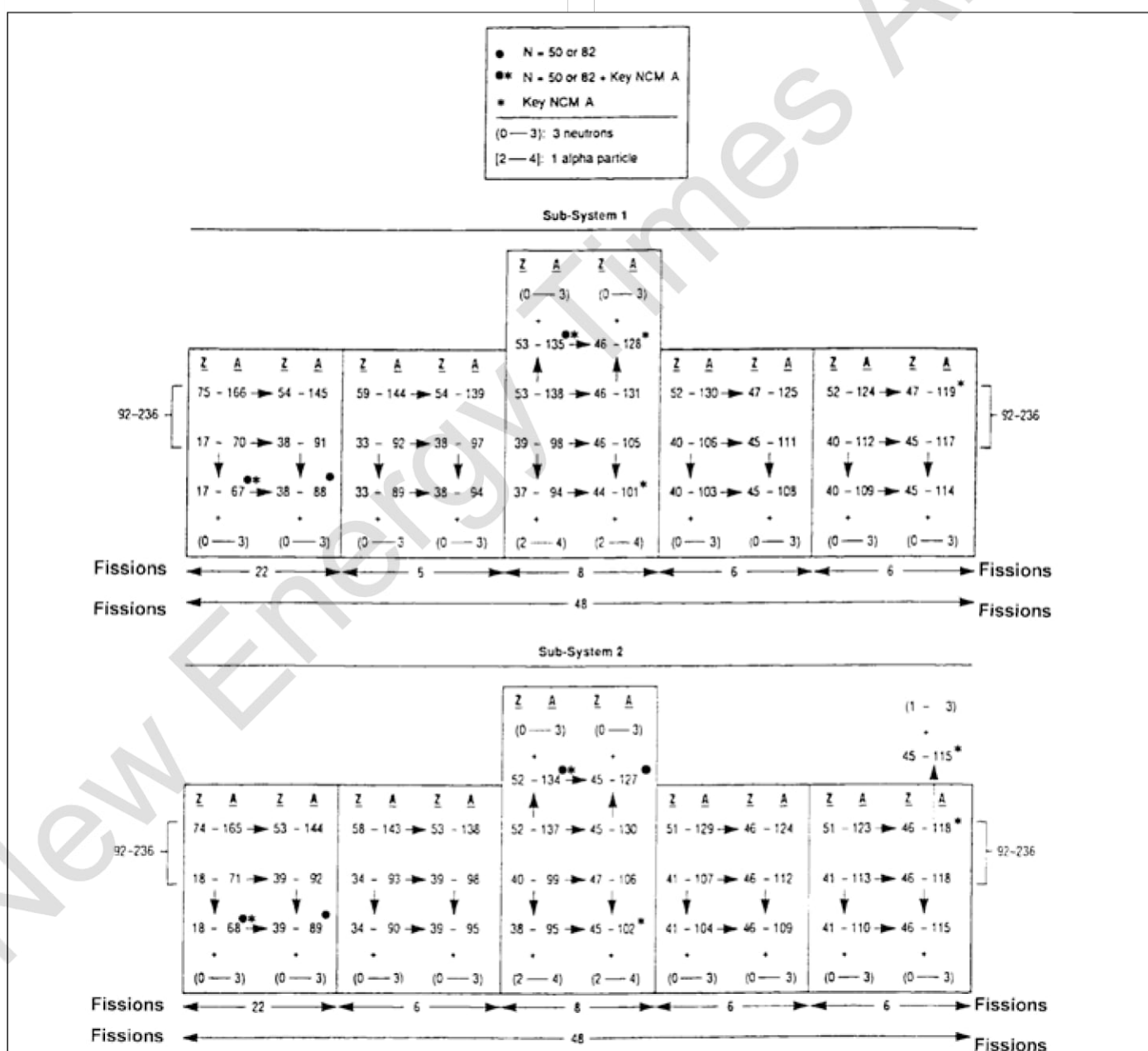


Figure 6. Thermal Neutron Fission of U-235. Fission Mode D.

Mode D: This mode was postulated and developed to account for the origin of alpha particles and tritons in fission. Two key experimental results had to be incorporated: 1) Alpha particles are observed in about one out of every 300 to 500 fissions. 2) The experimental ratio of alpha particles to tritons is extremely close to sixteen.

The model developed for Mode D consists of two sub-systems, each of which produces forty-eight fissions and three neutrons per fission. Each sub-system releases eight alpha particles, producing an alpha particle per fission ratio of 16/96 (.16667) for Mode D. In the unique case of completely symmetric fission, in which the mass and charge of both fragments are identical (46-118 + 46-118), one fragment emits three neutrons to become 46-115, while the other fragment emits a triton, to become 45-115. Fission Mode D is shown in Figure 6. In summary, Mode D not only emits three neutrons per fission, but also sixteen alpha particles and one triton. Thus, the NCM produces an alpha particle to triton ratio of sixteen, in excellent agreement with the experimental value⁷ of 15.9-16.1. Careful examination of Figure 6 will reveal that key NCM A values, as well as N = 50 and N = 82, play a significant role in Mode D.

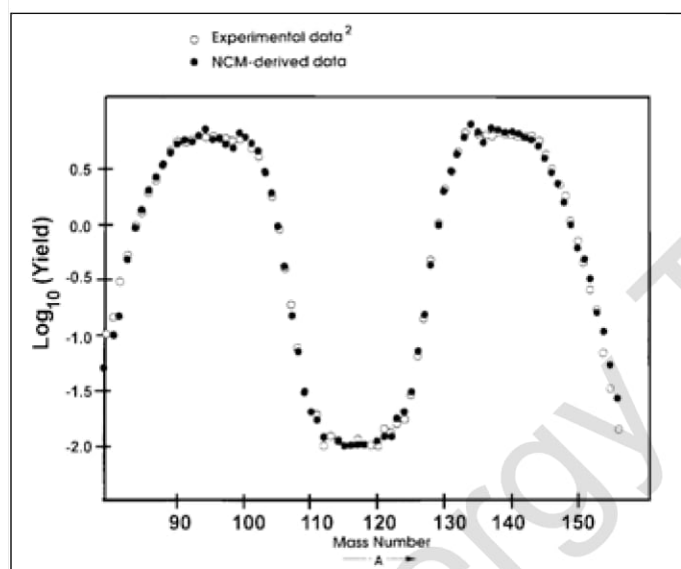


Figure 7. Fission Yields for Thermal Neutron Fission of U-235.

Derivation of Fission Yield Curve for U-235, $\bar{\nu}$, and the Alpha Particle Yield

Using the final fragment mass ranges for each mode, neutron emissions from the four modes, and experimental fission yields,³ as well as a constraint that $\bar{\nu}$ be very close to 2.416, a preliminary fission yield curve was derived (Appendix A) and is shown in Figure 7. The yield percent attributable to each mode was calculated as follows:

Fission Mode	ν	Final Mass Range		Percent Yield	$\bar{\nu}$
		Light	Heavy		
A	1	66-102	133-169	49.73	.4973
B	2	99-115	119-135	27.40	.5480
C	6	84- 96	134-146	22.83	1.3698
D	3	67-115	118-166	.04	.0012
TOTAL				100.00	2.4163

Since the percent yields for each fission mode can, of course, be expressed as integral frequencies, research was initiated to see whether the percent yield information shown above could be expressed as integral frequencies, resulting in:

- The correct value, within experimental error, of $\bar{\nu}$ for thermal neutron fission of U-235 ($2.416 \pm .008$).
- The approximately correct value, within experimental uncertainty, of the number of alpha particles per U-235 fission (one alpha particle in every 300-500 fissions).

For integral frequencies it is evident that f_D (the frequency of Mode D) cannot be less than one.

Two other observations were made, based on the percent yield data above:

- The ratio of the fission yield for Mode B (27.40%) and that for Mode C (22.83%) is exactly 1.20.
- The sum of the fission yields for Modes B and C is 50.23%, very close to the fission yield of Mode A (49.73%). This led to the postulate that the sum of the fission yields of Mode B and C equals the fission yield of Mode A.

Thus, the following frequency conditions were established:

- All frequency values are integral
- $f_D = 1$
- $f_B / f_C = 1.20$
- $f_B + f_C = f_A$

Fission Mode M	ν_M	f		$f \nu$	f		$f \nu$	f		$f \nu$	f		$f \nu$	f		$f \nu$
A	1	11	11	22	22	33	33	44	44	55	55					
B	2	6	12	12	24	18	36	24	48	30	60					
C	6	5	30	10	60	15	90	20	120	25	150					
D	3	1	3	1	3	1	3	1	3	1	3					
Sum		$\Sigma_f = 23$	$\Sigma_{f\nu} = 56$	$\Sigma_f = 45$	$\Sigma_{f\nu} = 109$	$\Sigma_f = 67$	$\Sigma_{f\nu} = 162$	$\Sigma_f = 89$	$\Sigma_{f\nu} = 215$	$\Sigma_f = 111$	$\Sigma_{f\nu} = 268$					
$\bar{\nu} = \Sigma_{f\nu} / \Sigma_f =$		2.43478		2.42222		2.41791		2.41573		2.41441						
Acceptable*		No		Yes		Yes		Yes		Yes						

*Within limits of error for $\bar{\nu} = 2.416 \pm .008$.

Figure 8. Thermal Neutron Fission of U-235. Calculation of $\bar{\nu}$ for integral frequency sets within required frequency conditions.

Figure 8 shows the $\bar{\nu}$ values calculated for five possible Σ_f values (23, 45, 67, 89, and 111). As can be noted, all frequencies except $\Sigma_f = 23$ result in $\bar{\nu}$ values within experimental error.

To select the correct Σ_f , the alpha particles per fission (α/Σ_f) have been calculated using the following simple relationship:

$$\alpha/\Sigma_f = (f_D/\Sigma_f) (\alpha/f_D)$$

Σ_f	f_0/Σ_f	α/f_0	α/Σ_f	Acceptable*
23	1/23	16/96	1/138	No
45	1/45	16/96	1/270	No
67	1/67	16/96	1/402	Yes
89	1/89	16/96	1/534	No
111	1/111	16/96	1/666	No

*Within experimental limits of error (400 ± 100).

Figure 9. Thermal Neutron Fission of U-235. Calculation of the number of alpha particles per fission for the five values of Σ_f in Figure 8.

where α/f_D has previously been shown (see Mode D) to be 16/96. Figure 9 shows the results of these calculations. Only $\Sigma_f = 67$ provides for the correct solution for both $\bar{\nu}$ and α/Σ_f . (See A) and B) above.) It is striking that the total frequency of the four fission modes should be 67, a value identical to the key mass number in the NCM.

Figure 10 shows the final NCM-generated fission yield curve (solid circles) using $\Sigma_f = 67$, as well as the experimentally deter-

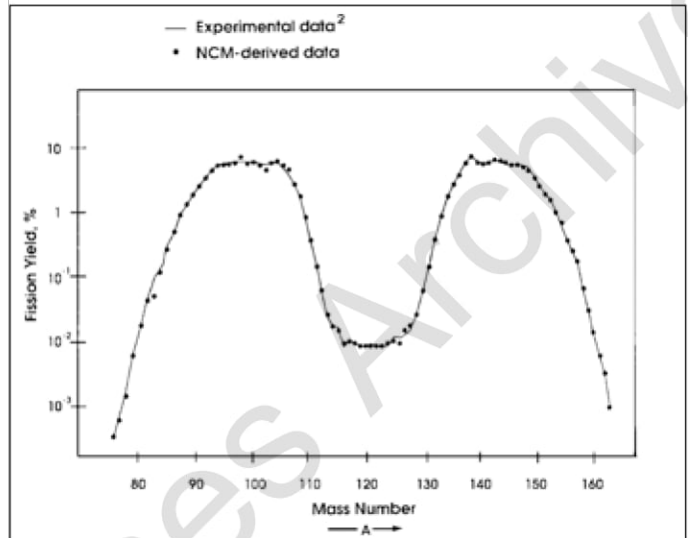


Figure 10. Fission Yields for Thermal Neutron Fission of U-235.

Fissile Nuclide	Experimental $\bar{\nu}$		NCM Fission Mode Frequencies				Σ_f	NCM $\bar{\nu}$
	Value ³	Range	A	B	C	D		
U-233	$2.492 \pm .008$	(2.484 - 2.500)	31	11	13	12	67	2.493
U-235	$2.416 \pm .008$	(2.408 - 2.424)	33	18	15	1	67	2.418
Pu-239	$2.904 \pm .008$	(2.896 - 2.912)	18	24	18	7	67	2.910
Pu-241	$2.947 \pm .007$	(2.940 - 2.954)	16	23	17	11	67	2.940

Figure 11. Thermal Neutron Fission. Variation of $\bar{\nu}$ with fissile nuclide mass number A.

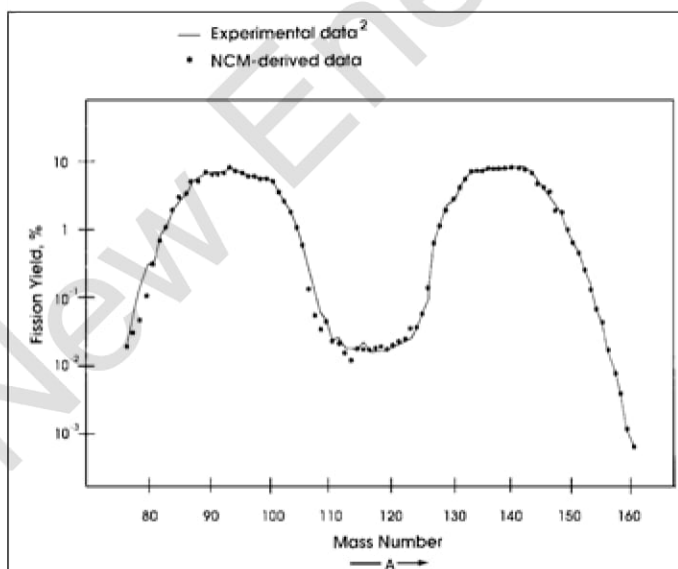


Figure 12. Fission Yields for Thermal Neutron Fission of U-233.

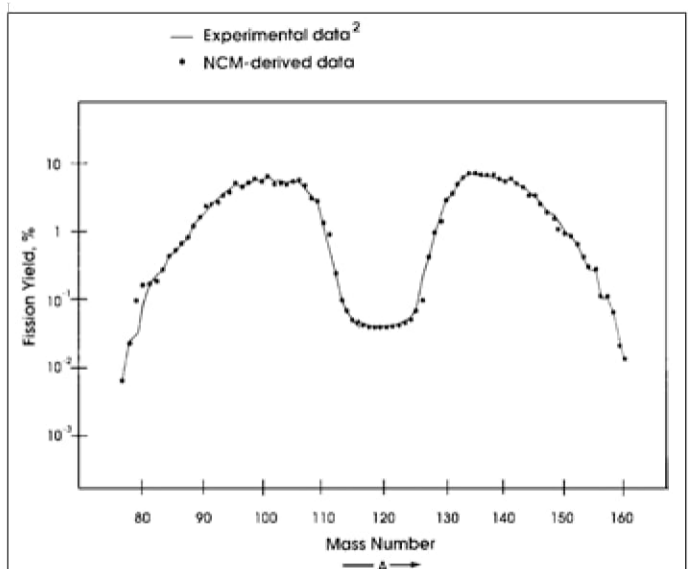


Figure 13. Fission Yields for Thermal Neutron Fission of Pu-239.

mined fission yield data (computer-trace line). As can be noted, the agreement is excellent, especially when it is recognized that experimental fission yields are, in most cases, uncertain by ± 10 -20%.

Thermal Neutron Fission of U-233, PU-239, and PU-241

Further research, utilizing the uniquely determined frequency $\Sigma_f = 67$, the four NCM fission modes, and the appropriate fission yield curves, has produced $\bar{\nu}$ values in excellent agreement with experimental values for U-233, Pu-239, and Pu-241, shown in Figure 11. Examination of mode frequencies for these fissile nuclides shows that, while Σ_f is constant, mode frequencies are shifted. The corresponding fission yield curves, also in remarkable agreement with experimental data, are shown in Figures 12, 13, and 14.

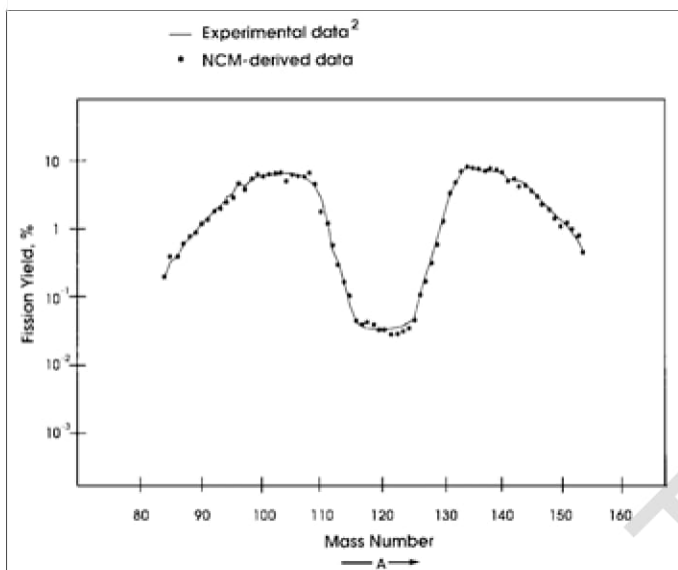


Figure 14. Fission Yields for Thermal Neutron Fission of Pu-241.

Conclusions

The NCM has been successfully utilized to derive a completely systematic model for thermal neutron fission of U-235 which:

- 1) reproduces the experimental fission yield curve;
- 2) reproduces the experimental value of $\bar{\nu}$;
- 3) accounts for neutron emission ranging from 0 to 6, and identifies the source of all prompt fission neutrons;
- 4) identifies the origins of alpha particles and tritons [the principal light charged particles (LCP)] and produces excellent agreement with the experimental ratio;
- 5) produces the approximately correct yield of alpha particles in fission;
- 6) shows the prominent influence of $N = 50$ and 82 .

The most significant conclusion from this research is that the Nucleon Cluster Model has been tested and derives, for the first time, key experimental results in thermal neutron fission, not only in U-235, but also in U-233, Pu-239, and Pu-241, never before explained. Additional papers validating the NCM are in preparation.

Acknowledgments

Dr. Frederick Forscher, a Director of NSRC, provided stimulating and creative suggestions during this research, as well as a critical review of this paper. Mr. Charles McNeill, a graphics consultant to NSRC, produced all figures in this paper, and his contributions are gratefully acknowledged.

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[Editor's Note: R.A. Brightsen was unavailable to clarify discrepancies in References 4 and 7. Our search found that these authors published separate books on nuclear fission. R. Vandenbosch is sole author of *Nuclear Fission*, which is out of print. It appears that J. Huizenga was contributor or editor for the proceedings of the International Symposium on Nuclear Fission and Heavy-Ion-Induced Reactions, which is also entitled *Nuclear Fission*.]

Appendix A. Derivation of Fission Yield Curve for U-235

Call the distribution curves for the individual modes the A-curve, the B-curve, the C-curve, and the D-curve. Since Mode A emits one neutron, the A-curve is symmetric around 117.5 (one-half of 236-1). Similarly, the B-curve is symmetric around 117, the C-curve around 115, and the D-curve around 116.5. The total yield curve is the sum of these four curves.

The restrictions on the curves are the following: They must yield the desired total distributions for A, B, C, and D. They are defined only in the appropriate ranges (for example, the C-curve only runs from $A = 84$ to $A = 96$ and, by symmetry, from $A = 134$ to $A = 146$). It is also reasonable to require that the curves be fairly smooth, having only minor local variations.

It is easy to see that the above restrictions are fairly rigid. For example, if the percentages of C are made larger, then the percentages for A for the same mass numbers must be made smaller. Since these mass numbers form most of the peaks, there is not enough yield elsewhere to make A large enough to compensate. Since six neutrons are produced in Mode C, while only one is produced for each fission in A, this would raise the value of $\bar{\nu}$. Correspondingly, lowering the percentages for C would tend to lower $\bar{\nu}$. It follows that the values for C should be moderately large. When these principles are carried out numerically, then adjusted to get a better fit, it is seen that the total percentage for C must be approximately the 22% given. Likewise, though B has a large range, most of it lies in a region where the experimental yield is small. The part of the curve corresponding to the peaks is for yields that must be shared with A. Again, as above, the balance between the two modes is fairly rigidly determined by $\bar{\nu}$. The fact that the different curves have different centers of symmetry plays a major role in the final determination of the percentages. For example, suppose the value for B for mass 99 is raised. Then the percentage for A for mass 99 must be lowered. By symmetry, the value for B for mass 135 must be raised and the value for A for mass 136 must be lowered. This latter change can be offset by raising the value of C for mass 136, hence by symmetry also raising the value for C for mass 94. The value of A for mass 94 can now be lowered, etc. So, a change in one value results in a change in many values. This introduces a lot of rigidity into the system, and causes the final distributions of percentages to be determined fairly accurately.

One feature of putting together four curves with different centers of symmetries is that the sum of the four curves will not be smooth, and will have local variations such as those occurring at the peaks. These irregularities have been observed experimentally and are not explained by other models.

The overall conclusion is that it is possible to have four modes as

described, each with its own distribution curve, and still be consistent with the experimental yield curve. The rigidity of the situation, as described above, indicates that this consistency would probably fall if variations were to be introduced in the individual modes.

About the Author

As a scientist and business executive, Ronald A. Brightsen has been actively involved for many years in military and civilian uses of nuclear physics. During World War II he worked as an analytical chemist on the first atomic weapons project at Oak Ridge. He also participated in the 1948 atomic weapons test in the South Pacific, and later helped establish the project that detected the first Russian atomic weapons test in 1949. As a senior scientist at the Atomic Power Division of Westinghouse, he headed radiochemical experiments in conjunction with development of the first nuclear-powered submarine and the first U.S. civilian nuclear power plant. In 1954, Brightsen established the Nuclear Science and Engineering Corporation, the nation's first commercial enterprise aimed at developing the industrial application and production of radioactive isotopes. In more recent years, he held several senior positions at the U.S. Nuclear Regulatory Commission. In 1985 he created Nuclear Science Research Corporation (later to become Clustron Sciences Corporation), in order to further develop his personal research on nuclear structure; this research ultimately led to the creation of the Nucleon Cluster Model. In 1985, he retired from the NRC to devote full-time attention to the development of the science and potential practical applications of the Nucleon Cluster Model.

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Irrational opposition must be overcome every time someone with an innovation appears on the scene.

—Jed Rothwell

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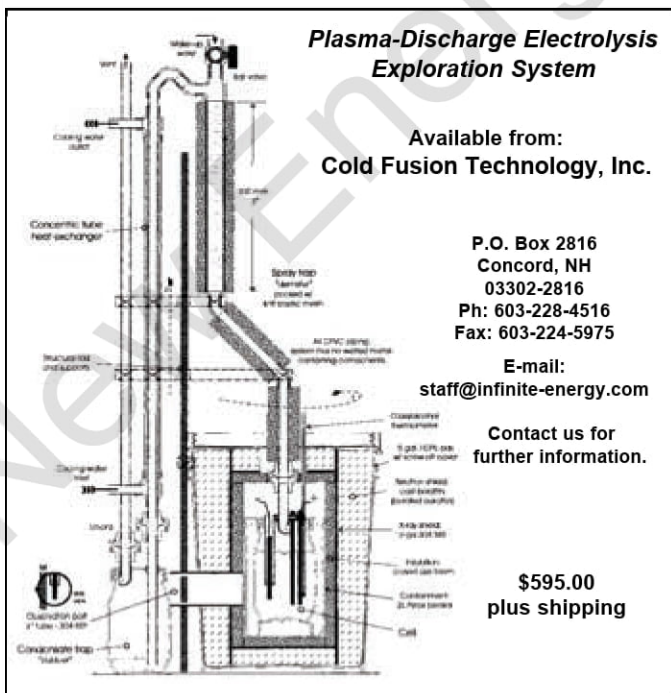
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PD Black continued from page 53

marks like the wire. In contrast, the enclosing stainless steel tubing was easily wiped clean to show a bright metal surface. A subsequent test in which powder was introduced into the test cell with no piece of Pd metal, but was otherwise loaded with deuterium at ambient temperature as previously done, showed no evidence of metallic grains on disassembly.

The above observation seems to support the Arata and Zhang view that an assembly of Pd black grains in direct contact with an enclosing Pd metal surface in the presence of D₂ gas can develop an integral contact between the metal surface and the enclosed Pd grains, even at ambient temperature. This does not necessarily mean that the powder is the source for all the heat produced in the DS-cathode studies, but it supports the view that the D-chemical potential of the grains inside the DS-cathode more closely matches that of the bulk metal than that of the contacting gas.

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In Memoriam
Thomas J. Higgins
Cartoonist for *Infinite Energy*
November 21, 1922 - March 11, 2000

Gamma Decay Control and Cold Nuclear Fusion Are the Two Yields of the Controlled Rheological Process Application

V.I. Vysotskii* and Y.A. Kornienko**

The search for solutions to energy production problems and enforced waste utilization (EWU) problems from cold nuclear fusion (CNF) can be connected with the experimentally observed elements of transmutation in cathode matrices, on the one hand, and with the energy-profitable nuclear fusion process, on the other hand.

The difficulties of these problems' solution arise from the search for the effective action of nuclear fusion or fission operating due to the possibility of gamma decay control (GDC). The possibility of such action, as it will be shown below, can be expected from the controlled rheological process (CRP) in the solid matrix due to "virtual microvacuum pores" (VMP) arising.

The controlled rheological process in a solid matrix is connected with the matrix material deformation, which produces the breaking and the reconstruction of interatomic bonds in the matrix, i.e. to generate the VMP in the matrix. The CRP conditions can be realized for more than twenty metals, for example, palladium (Pd), copper (Cu), or aluminum (Al). The VMP generation process can be observed practically in matrices, which have initially separated fragments which join into unit sample, adequate to the continuous ones. The particular case of the CRP can be presented by cold welding (CW) of metals, which can be successfully used for creating complex matrix constructions for CNF and EWU.

The CRP operating conditions are chosen in order to produce the dihedral stress σ_{ik} and the quasi-viscous metal flow in the dihedral direction. Under this situation in the range of the great stress gradient, the conditions are realized for the arising of micropores; their internal surfaces immediately collapse without defects, if their sizes are less than some critical value. The CRP operating conditions can be chosen experimentally according to the matrix material and construction. The specific mechanism of the origin of micropores can be different.

In an ideal uniform medium it can occur due to the fluctuation process, realized with the probability $f \approx \exp(-E_0/KT)$, where E_0 is the threshold energy of birth of a micropore and T is the absolute temperature of a medium.

The E_0 value sharply decreases under the stretching stress or the local imperfection (dislocation, vacancy). In the last case, the micropore origin process can be satisfactorily described by the dilatometric mechanism, which is provided by the self-amplified process of the phonon spectra localization in the embryo region due to the reduction of the speed of the elastic waves in this place.

The further evolution of any micropore is determined by the balance of acting forces. Due to the stress limitation condition at the micropore boundary, from the quasi-equilibrium microcrack theory¹ one obtains the requirement:

$$2L = 4\alpha E / \pi(1-\sigma)p_0^2 \quad (1)$$

It connects the micropore length $2L$ with the surface strain coefficient α , Young's modulus E , local stretching stress $\sigma_{yy} = p_0$ and Poisson's ratio $\sigma = -U_{xx}/U_{zz}$.

The relation $2L \sim 1/p_0^2$ shows that due to p_0 increasing, the micropore size decreases and then it collapses. As a result of such a deformation process, the medium gets rid of the continuously generated micropores (self-curing). In order to provide the stability of this process, the stress must satisfy the relation:

$$p_0 \approx p_d = [4\alpha E / \pi(1-\sigma)d^2]^{1/2} \quad (2)$$

where the equilibrium size $2L$ refers to the interatomic distance d .

In the described CRP scheme, four items are of great importance.

- 1) The origin of "virtual microvacuum pores" in the continuous matrix.
- 2) During the origin and collapse, the size of every micropore is continuously changed, inevitably "passing through" the whole interval of values required for the CNF and GDC realization.
- 3) The accidentally arising and collapsing micropores cover statistically the whole region of the p_0 action.
- 4) The collapse rate depends on T and p_0 , which let us control the rheological process.

We can show that the required factors of CRP allow us to produce CNF and GDC.

It is known that to explain the CNF mechanism and to optimize it, one should determine the non-tunneling mechanism of the "elimination" of the d-d interaction Coulomb barrier. It was shown²⁻⁴ that one of the most probable non-barrier CNF realization mechanisms can be connected with the fluctuation-nonstationary ($N = 10-20$) fermi-condensation of deuterons in the optimal size micropores⁴ (microspheres, radius $R_0 \approx 4-7\text{\AA}$, and microcracks, middlewidth $l_0 \approx 3-4\text{\AA}$).

CNF is caused by the quantum mechanical probability of the "elimination" of the Coulomb interaction between deuterons. Such an effect takes place only when the energy of inter-deuteron interaction $V(|\vec{r} - \vec{r}_m|)$ in systems (d_n-e-d_m-e) is a negligibly small disturbance in relation to deuteron interaction with the quantum field of the micropore surfaces.

It was shown²⁻⁴ that under the conditions $V_{nn} \approx 0$, $|V_{nm}/(E_n - E_m)| \ll 1$, every deuteron movement in the micropore becomes mutually independent. Here V_{nn} and V_{nm} are diagonal and nondiagonal matrix elements of interaction energy; E_n and E_m are the energy of levels in its localization range, determined only by the individual wave function $\Psi_n(\vec{r})$ and does not depend on the other particle localization in this micropore.

Under these conditions, the time of the d-d fusion is equal to²⁻⁴

$$\tau_{dd} = [C \int |\Psi_n(\vec{r})|^2 |\Psi_m(\vec{r})|^2 dV]^{-1} \approx 1/CV_0 \approx 10^{-8}s \quad (3)$$

where V_0 is the volume of optimal micropore, $C = 2 \cdot 10^{-16} \text{ cm}^3 \text{ s}^{-1}$ is the constant of a d-d nuclear interaction, and $\Psi_n(\vec{r})$ is the wave function of particle in state n in the micropore. The same requirement of the use of the micropotential well with necessary critical size and structure arises also under the modeling of the conditions for the nonstationary d-d CNF in the system of only two deuterons and one electron (d-e-d) in the same parabolic well $U(r) = M\omega_0^2 r^2/2$. Here M is a mass of deuteron, and ω_0 is the resonant frequency in a parabolic well. For the nonstationary case, the energy of d-d interaction has the form $V(|\vec{r}|, t) = V(r)F(t)$. Here $F(t) = 0$ at $t < 0$, $t > t_0$, and $F(t) = 1$ at $0 \leq t \leq t_0$. Such a relation (d-e-d) is usual for Pd.⁵ In this case together with the interlayer transition restriction requirement,

$$W_{nm, kp} = \hbar^{-1} |V_{nm, kp}|^2 \int_0^{t_0} F(t) \exp(iN\omega_0 t) dt = 0 \quad (4)$$

(which is provided under the certain temperature T_{opt} or the certain limited time t_0 of the deuteron presence in the pit together with the other deuteron) also arises the requirement $V_{nm,nm} = 0$, which is realized taking into account the sign variable character $V(r)$ only in the micropore with the precisely determined sizes. According to the valuation,⁶ it is necessary to use the well with $R_0 \approx 1-1.5\text{\AA}$ and the parabolic form of the potential. Under the mentioned conditions, the d-d reaction time refers to $\tau_{dd} \approx 10^{-8}$ s. It should be noted that for non-parabolic form $U(r)$, the optimal size R_0 characterizes only the central lot of the pit [near $U(0)$], which is parabolic.

In all cases these critical sizes must have certain values R_0, l_0 , with the small allowed variations³: $\Delta R/R_0, \Delta l/l_0 < 0.1$. It is important that the precise value of the micropore size cannot practically be calculated under the uncertain form different from a sphere or a flat crack. Obviously, for the reliable realization of the non-barrier CNF conditions, it is desirable to have a mechanism for the rather slow (compared to the reaction time τ_{dd}) change of R, l , in order to realize the self regulation $R, l \rightarrow R_0, l_0$ (in the course of the reaction time τ_{dd}). It can be shown that the analogous requirement of the origin of the micropores with precisely determined sizes is one of the conditions of the decay rate control for the excited nuclei. For Mössbauer radiation such control is possible due to the use of frequency-selective shells, screening the excited nuclei.⁷ Another situation has a place for the non-Mössbauer radiation.

It is known that the spontaneous decay of gamma-active states of any quantum system is determined by the referenced system characteristics as well as by the electromagnetic vacuum properties. Every field mode with frequency ω in the ground (vacuum) state is characterized by the energy density $u = \hbar\omega/2V_0$. Here V_0 is a volume of quantization.

The spontaneous emission probability in the dipole approximation per unit time due to the interaction of the excited quantum system with the separate field mode with frequency ω is equal to

$$P_{sm}^{(1)} = (4\pi^2\omega |\vec{d}_{sm}|^2 / 3\hbar V_0) \delta(\omega_{sm} - \omega) \quad (5)$$

where $\vec{d}_{sm}$ is the matrix element of the gamma transition dipole moment.

The same relation will be valid for the high multipole transitions. In the case of great volume $V_0 \gg c^3/\omega^2\Gamma = V_\omega$, the resulting expression for the total spontaneous transition probability P_{sm} can be obtained from $P_{sm}^{(1)}$ by summing over all modes with the use of their distribution law $\rho(\omega) = dn/d\omega = \omega^2 V_0/\pi c^3$ and then

$$P_{sm} = 4(\omega_{sm})^3 |\vec{d}_{sm}|^2 / 3\hbar c^3 \quad (6)$$

The other situation takes place in the case of the small quantum volume $V_0 < V_\omega$. In this situation, due to the sharp increasing of the intermode distance, the nucleus can interact only with one specific mode and the transition probability is directly determined by the expression $P_{sm}^{(1)}$ with the substitution $\delta(\omega_{sm} - \omega) \rightarrow (\Gamma/2\pi)/[(\omega_{sm} - \omega)^2 + \Gamma^2/4]$.

Thus, we have found that due to the $(\omega_{sm} - \omega)$ value, acceleration will occur when $(\omega_{sm} - \omega) < \Gamma/2$ and braking will occur when $(\omega_{sm} - \omega) > \Gamma/2$ of the spontaneous decay.

It should be noted that for small micropores, their volume does not coincide with the mode localization volume, which is rather greater. Besides, in most cases (especially under very high gamma-transition frequencies), the mode width Γ , even in the lowest part of spectra, can prevail over the intermode distance.

Summing the conditions of the CNF and GDC realization considered, we have to note that they essentially coincide with

the CRP realization conditions. Thus, under the deuterium or excited nucleus loading of the sample, which is under external action, in order to provide CRP realization, there is a possibility to realize CNF and gamma decay as the controlled processes.

The practical realization of the method described depends on the matrix material and its construction and experimented protocol needed.

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Cold Fusion: The Reproducibility Problem

Howard T. Francis*

Having recently re-read the "Review of the Sixth International Conference on Cold Fusion (ICCF-6)" by Jed Rothwell,¹ I resolved to summarize my thoughts on the subject, accumulated over the years since 1989. My view on cold fusion (CF) is neither "believer" nor "non-believer," but merely that of a curious retired electrochemist who views "non-reproducibility" as a challenge—not the "kiss of death."

I hasten to point out that I have never carried out a CF experiment (retired since 1984). I was, however, intensely interested when I read the initial news releases and found that the Vice President for Research at the University of Utah was an old friend and co-worker—the late Dr. James J. Brophy. We had cooperated on several projects in the 1950s when we were both at Armour Research Foundation (now ITRI) in Chicago. He was a much-respected solid-state physicist, while my field was the electrochemistry of metals.

I wrote to Jim Brophy almost immediately and carried on brief technical correspondence. However, shortly after the creation of the Utah research group, Dr. Brophy's death, and the fact that I did not know either Fleischmann or Pons personally, I cut off further informal contact.

Introduction

Before proceeding, it is necessary to concede that all of the comments that follow are based on a single assumption: that excess heat has, in fact, been produced in some CF experiments. Based on our confidence in chemical thermodynamics—and barring gross incompetence on the part of able and honest experimenters—we are invited to the further hypothesis that the heat came from a nuclear reaction (of a sort certainly not recognized by conventional physics theory).

In the discussion below, I have three goals: 1) to suggest a possible explanation for the non-reproducibility of Pons-Fleischmann (P-F) type experiments using solid palladium electrodes and deuterium-rich electrolytes, 2) to compare D and H as reactants, and 3) to suggest an approach to achievement of the so-called "special state of grace" in which CF can occur.

Metal-Hydrogen Solid Solutions

It is appropriate to review briefly the creation of solutions of hydrogen in various metals. Most metals are not at all receptive to hydrogen as a solute; some, however, readily dissolve elemental hydrogen. The latter include iron, nickel, and palladium.

An old problem in ferrous metallurgy illustrates the mode of entry of hydrogen as well as its effect on the integrity of the metallic structure. It is called "hydrogen embrittlement" and occurs when certain steels (usually rather high-strength alloys) are subjected to "hydrogen-charging" by an electrochemical process. That process may be simply "pickling" the steel in an acid solution to remove surface oxides or by "electrocleaning" in an alkaline solution (degreasing) prior to an electroplating sequence. Finally, the plating operation itself may cause hydrogen entry, particularly when the plating solution does not deposit metal at 100% faradaic efficiency, that is, when there is simultaneous reduction of hydrogen ion along with the metallic ion being plated.

Steels that were thus charged with hydrogen frequently failed (by cracking) under service loads normally well within the expected strength of the alloy. The accepted theory of failure was this: 1) atomic hydrogen in solution migrated freely through the steel, 2) if any voids were present in the lattice, hydrogen atoms

combined to form hydrogen gas (molecules) within the voids, 3) the high gas pressure produced stresses which caused micro-cracks to initiate, and 4) failure in service was caused by the propagation of these cracks (the "notch effect").

Palladium-Hydrogen: Established Facts

Before tackling the CF reproducibility puzzle, it is appropriate to review the well-known and generally accepted phenomena associated with the Pd-H system.

1) A hydrogen ion can be reduced electrolytically at a Pd surface, producing "nascent" H that either a) dissolves in the Pd or b) pairs with another H to form molecular hydrogen that escapes into a gas phase. The "efficiency" of a) varies widely, depending on a myriad of surface conditions that we need not pursue in detail.

2) After H is dissolved in the Pd, it migrates quite freely in the Pd lattice. I rely on Dr. Brophy's view² that the H atom is actually present as a H (+) nucleus, with its associated electron (-) becoming part of the free electron population of the Pd metal.

3) The hydrogen in the metal lattice will be retained in solution until it encounters a phase boundary that permits formation of a gas phase. That is, if the dissolved H reaches an external surface that is *not* subject to the electrochemical field that caused its initial dissolution, it will vigorously escape from that surface as hydrogen molecules (gas).

This "free-escape" condition will apply at the electrolyte surface when the "charging current" is turned off, but—and this is the critical point in my whole proposal—it will also exist at any *internal* metal-gas boundary *within* the metal lattice, even when the charging potential is connected and hydrogen atoms are entering the metal at the electrode surface!

4) Inasmuch as deuterium is merely a chemical isotope of hydrogen, we have every reason to believe that deuterium may be introduced into a palladium lattice by precisely the same electrochemical mechanism as just described for H. Furthermore, the freedom of diffusion and the paths for escape from solid solution must be nearly identical to those for hydrogen.

Palladium-Deuterium: Failure Hypothesis

If the P-F experiments produced heat by deuterium nucleus fusion, the possibility has been suggested that helium was produced, together with thermal energy. I suggest this possible sequence of events: 1) the energy released locally weakens the metal lattice; 2) the helium produced is extremely *insoluble* in Pd; 3) the combination of these two effects causes local rupture of the Pd lattice, producing micro-voids in the metal; 4) the dissolved deuterium atoms instantly begin combining to form molecular deuterium gas in the voids, leading to very high gas pressure and further cracking throughout the metal and 5) the concentration of *dissolved* D atoms drops precipitously, and heat effects disappear.

One might also expect that if Pd metal from such an experiment were *reused* without processing to regain its original structure (no voids), negative results would be inevitable. That is, any microscopic voids *within* the metal would serve as "sinks" for dissolved (atomic) deuterium, forming *molecular* deuterium that would certainly not contribute to the desired fusion reaction. While deuterium ions could still be reduced and dissolved at the *surface* of the electrode, they would be lost rapidly as they diffused to *internal* surfaces not in contact with the electrolyte.

Palladium-Hydrogen: Success Hypothesis

Recalling the reported reproducibility claimed for the Patterson cell as compared to P-F cells, consider this: if the reported excess heat is produced by H fusion, a simplistic view is that the fusion product could be deuterium. If a deuterium nucleus were formed within the Pd lattice, one would expect that it *would remain in solid solution!* Even if large amounts of thermal energy accompanied such a reaction, there should be no lattice-stressing product formed, since deuterium atoms are chemically the same as hydrogen atoms and would be readily soluble in the Pd lattice.

I do not mean to imply that the Patterson approach is ideal—merely that the crack-generation mode of lattice destruction is not present. If hydrogen atoms can be used as the reactive species, the crack generation discussed above may be avoidable—more later.

Electrical Conduction in H-Charged Palladium

As mentioned above, accepted theory is that dissolved hydrogen (or deuterium) is present as positive nuclei, with the electrons joining those from the metal itself. Hence, we have a structure containing *two* highly mobile charged particles, both + and -. Of course, the electrons will be much more mobile than the positive nuclei; but, since both are free to move, conduction of a DC current can occur by movement of either charge.

The situation ought to be similar to conductivity in ionic solutions, where the + and - carriers each transport part of the total current. The proportion carried by each is referred to as the “transference number” for that ion. In the case of potassium chloride solution, the number is roughly 0.5 for each ion. With acids, on the other hand, nearly all of the current is carried by the hydrogen (+) ions.

One might expect that a Pd lattice containing dissolved protons (or deuterons in the P-F cell) would behave in a similar fashion, with the transference number for the electron being nearly unity and that for the proton (or deuteron) being small. Even with a very low contribution to the conductivity, however, one might expect a significant degree of migration of the positive charges.

The proposed scheme is simply to pass a DC current through a solid palladium structure (wire, foil, rod, or bar) that is *simultaneously* subjected to hydrogen charging. The two currents need not be the same, of course.

Requirements for Useful Power Generation

It seems clear that the ultimate success of cold fusion will depend on the temperature level that can be achieved in an actual power-generating device. In conventional fossil fuel plants the steam temperature at the turbine input is in excess of 1000°F, leading to a reasonable thermodynamic efficiency, $(T_2 - T_1)/T_2$. Similarly, fission reactors have been developed with comparable thermal characteristics.

If fusion reactors are to reach the status of these commercial

power generators, we must learn how to: 1) charge the palladium with the appropriate reactive nuclei and 2) concentrate the reactive species in a region of palladium designed to facilitate rapid heat transfer to the surrounding fluid (presumably water).

The charging mechanism seems relatively straightforward, as mentioned earlier. The second problem appears to be the major one, since H (or D) will escape from any free surface of the Pd that is not at a negative potential with respect to an electrolyte solution (the “charging” condition).

With the background knowledge of hydrogen dissolution in other metals (above), we can predict that a Pd surface could be coated with a material that would prevent escape of atomic hydrogen atoms through recombination. Copper has been used for this purpose, but almost any metal in which H has negligible solubility should serve as well (e.g. lead or tin). Even more attractive would be a *non-metallic* coating (no electrical conductivity), such as a glass or ceramic that could be fused to the Pd surface with atomic-scale bonding (tightly adherent with no voids in the boundary zone).

Thus, it may be possible to a) *charge* hydrogen into Pd in a cell at room temperature (or a little higher), b) cause it to *migrate* from the charging region through a section coated with an “escape barrier” and finally c) *concentrate* it in a “fusion boiler” region (also coated to prevent molecular escape). If the reacting species is H⁺ rather than D⁺, destruction of the Pd should be avoided (as discussed earlier).

Summary and Conclusions

Very early in the history of cold fusion, the problem of reproducibility led to violent disagreement among scientists. In the initial burst of activity, many experimental observations were made that were interpreted to support or deny the reality of CF—depending on the camp to which the experimenter belonged!

One of these observations was that palladium electrodes contained tiny cracks. When I read the title on the initial report,³ my reaction was: “Aha! Now we know why the fusion process stopped!” But upon perusing the article further, I was astounded to find that the author proposed a theory in which the cracks were the *cause* of fusion. As discussed above, the presence of cracks (especially *micro*-cracks away from the surface) would most certainly lead to loss of D nuclei from the solid solution!

It should also be noted that very few workers seem to have embraced the question of electromigration of D⁺ (or H⁺) nuclei in Pd. At least it appears that it has not been pursued as a means of *concentrating* the reactive species in the lattice.

I recall one item that reported “melted solder connections” in a CF cell. This suggests to me that migration of deuterium nuclei may have occurred inadvertently. If the Pd cathode in that experiment had a solder-coated “tab” at

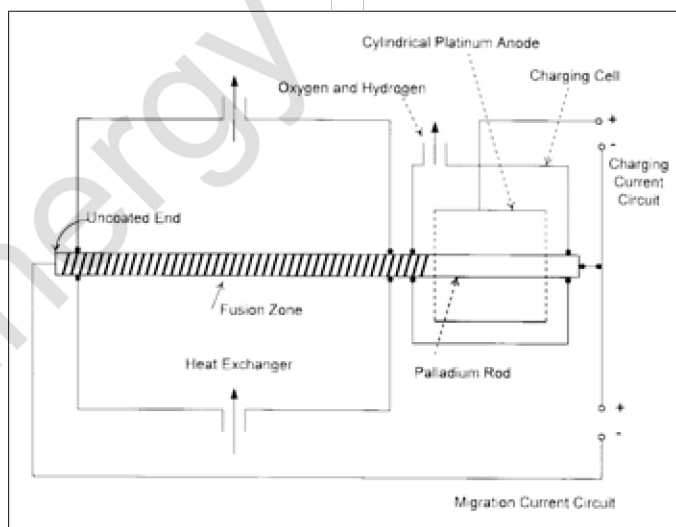


Figure 1. Schematic of “Linear Migration” Cold Fusion Device.

1) The region of the Pd rod (or wire) within the charging cell is bare metal—all exposed to cathodic current.

2) All other regions of the Pd are coated with a metal (or non-metal) with very low solubility for atomic hydrogen.

3) The charging current would be adjusted to achieve a suitable rate of entry of hydrogen into the metal.

4) The “migration current” would be adjusted, to produce the critical level of H for fusion within the heat exchanger region.

5) The negative end of the Pd rod could be left uncoated, as shown, so that some molecular hydrogen could escape, thus preventing appreciable deuterium accumulation (see text).

the point of connection to the external circuit, it is conceivable that the critical fusion concentration was reached at that point—especially if the tab were somewhat reduced in cross section. (The solder may have prevented escape of molecular deuterium gas.)

Further, it seems that many people think of the fission threshold as involving something other than simply reaching a critical concentration for reaction. In a review of the field,⁴ Pennisi used the words “triggering,” “sparking,” and “jolting” several times. In my view concentration is the critical factor.

In any case, I reiterate the view that “reproducibility” alone cannot be used to condemn experimental results. Finding the reason that an experiment does not always work may be a nobler goal.

Finally, Figure 1 shows a linear version of a cold fusion device comprising a “loading zone” for hydrogen atoms, a “transition zone” (to isolate the cooler charging region), and a “fusion zone” in which heat would be removed by circulating water. It is acknowledged that the refinement of this basic concept to power-plant scale would require some excellent engineering. Such development was successful with “cold fission”; the problems with fusion appear no more challenging.

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About the Author

After earning his Ph.D. in Physical Chemistry from Pennsylvania State College in 1942, Dr. Francis went on to become the Manager of Electrochemistry at IIT Research Institute (formerly Armour Research Foundation) until 1971. From 1971 to 1984 he worked for Cook County Hospital as the Director of Biomedical Engineering. His research interests over the years included corrosion, electroplating, batteries, and other areas involving the electrochemistry of metals. He is now retired.

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Erroneous Predictions in the History of Science

Excerpts from *Erroneous Predictions and Negative Comments Concerning Exploration, Territorial Expansion, Scientific and Technological Development*, Prepared at the request of the U.S. Senate Committee on Aeronautical and Space Sciences by Nancy T. Gamarra, April 10, 1967

On the limitations of airplanes:

“... The machines will eventually be fast, they will be used in sport, but they are not to be thought of as commercial carriers. To say nothing of the danger, the sizes must remain small and the passengers few, because the weight will, for the same design, increase as the cube of the dimensions, while the supporting surfaces will only increase as the square. It is true that when higher speeds become safe it will require fewer square feet of surface to carry a man, and that dimensions will actually decrease, but this will not be enough to carry much greater extraneous loads, such as a store of explosives or big guns to shoot them. The power required will always be great, say something like one horse power to every hundred pounds of weight, and hence fuel can not be carried for long single journeys.”

Chanute, O. 1904. “Aerial Navigation,” *Popular Science Monthly*, March, p. 393.

On opposition to the use of alternating current:

“... My personal desire would be to prohibit entirely the use of alternating currents. They are unnecessary as they are dangerous. . . I can therefore see no justification for the introduction of a system which has no element of permanency and every element of danger to life and property.”

Edison, T.A. 1889. “The Dangers of Electric Lighting,” *North American Review*, November, p. 632.

On Ford’s experiments with gas engines:

“The Edison Company offered me the general superintendency of the company but only on condition that I would give up my gas engine and devote myself to something really useful.”

Ford, H. 1922. *My Life and Work*, Doubleday, Page and Company, New York, p. 35.

Letter from Charles A. Lindbergh to Harry Guggenheim of the Guggenheim Foundation, May 1936:

“I would much prefer to have Goddard interested in real scientific development than to have him primarily interested in more spectacular achievements which are of less real value.”

Lehman, M. 1963. *This High Man: The Life of Robert H. Goddard*, Farrar, Strauss and Company, New York, p. 231.

John Aubrey, a contemporary, wrote this account of the response William Harvey received upon publication in 1628 of his book *De Motu Cordis*, in which he described his discovery of the blood’s circulation:

“... I heard Harvey say that after his book came out, he fell mightily in his practice. ‘Twas believed by the vulgar that he was crack-brained, and all the physicians were against him. I knew several doctors in London that would not have given threepence for one of his medicines.”

Williams-Ellis, A. 1930. *Men Who Found Out*, Coward-McCann, Inc., New York, p. 75.

U.S. Rear-Admiral Clark Woodward (1939):

“... As far as sinking a ship with a bomb is concerned, you just can’t do it.”

Woods, R.L. 1966. “Prophets Can Be Right and Prophets Can Be Wrong,” *American Legion Magazine*, October, p. 29.

I will take a typical example: it is well known that diesel fuel is a strong pollutant; now it has been demonstrated that biodiesel, made from vegetable oils and methanol/ethanol, is less polluting ("Emissions from Nine Heavy Trucks Fueled by Diesel and Biodiesel Blend without Engine Modification," Environmental Science Technology (ACS), 34 (9), 9333-9339, 2000, by W.G. Wang, D.W. Lyons, N.N. Clark, M. Gautam from the West Virginia University, Morgantown and P.M. Norton, National Renewable Energy Laboratory, Golden, CO). Significant reductions in particulate emissions (25%), for carbon monoxide (10-12%), and in hydrocarbons (10%) have been obtained. The value of this study depends on the extent at which "biodiesel" will replace its "mineral" counterpart.

After so many years in industry I have the vision of great progress being made from myriad such small steps. However, "quantum leaps" or breakthroughs are also necessary, and complementary to the former; it is a great error to think that these two ways or methods are contradictory.

In the spirit of this idea, it is my pleasure to call the attention of the readers to the best renewable energy and energy efficiency journals (the two subjects are difficult to separate!) published by Elsevier Publishers <www.elsevier.com>: entitled Applied Energy; Biomass and Bioenergy; Energy, Energy Conversion and Management; Energy Policy; Energy and Buildings; Fuel and Energy Abstracts; Geothermics; International Journal of Hydrogen Energy (this one quite friendly toward cold fusion, and, by the way, the July 2000 issue will contain Randell Mills' paper "Novel Inorganic Hydride"); Renewable Energy; Renewable and Sustainable Energy Reviews; Solar Energy On-line; and Solar Energy Materials and Solar Cells. These reflect a broad creative effort. Just by reading the titles of papers in each issue, which are sent to you due to a free subscription, you can learn a lot about problems, progress, and future trends. And speaking of Elsevier, it also runs *Journal of Electroanalytical Chemistry*, which hosted the very first cold fusion paper back in 1989 and many others up to today.

Small progress and great progress must live in harmony.



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current of electricity distributes itself near the end of a wire dipping in a mercury cup is gratifying in its comprehensiveness, but disappointing in that when we meet that function, we do not know which of the many possible phenomena it represents. Take, for instance, our equations for light. They fit in with a simple elastic-solid wave, and we have fallen into the habit of speaking of light as really being such a wave, and some eminent physicists, even, as I have pointed out elsewhere, have fallen into the mistake of supposing that the magnetic rotation of light necessarily implies a rotation of the medium in a magnetic field, overlooking the fact that the whole proof is based on this unfounded and, as we now know, certainly incorrect, supposition. All that the equations really mean is that light is some kind of periodic motion, but, if I remember rightly (as it was some years ago that I investigated the matter), there are eight kinds of periodic motion which can be equally well represented by the light equations.

3. *Ibid.* *Phys. Rev.*, also *Elect. World*, May 18, 1895.

4. Velocity is proportional to square root of density, but change of velocity is proportional to change in density, both being small.

5. *Elect. World*, May, 1900.

6. In a paper on Comets' Tails, *Astrophysical Review*, January, 1897, the writer showed that all the phenomena so far noted in this connection, including the bridge of Biela's comet, the apparent retardation, the shape, etc., could be accounted for by supposing that the ultra-violet light of the sun acted on the surface of the nucleus of the comet to throw off negatively charged particles. It is possible that this compressive wave may be a factor in this discharge, though on the other hand it is possible that the light itself may be sufficiently effective.

7. Ether is the structure formed by the fluid and the vortices, etheron the fluid alone.

8. *Elect. Soc.*, Newark, 1890; *Elect. World*, August 8-22, 1891.

9. *Ibid.*



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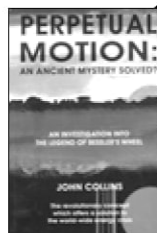
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The man from DaimlerChrysler seemed clear on the latter point: "You can't save or destroy the world with a vehicle," he interjected and went on to declare that the bulk of global pollution came from static sources. Fuel cell research, he said, was much more far-reaching than just vehicle power.

Perhaps a hint that DaimlerChrysler intends to move into the domestic power supply market—after they eventually pinpoint an appropriate FC fuel source, that is.

The remainder of the conference, much like last year, was taken up by various organizations, commercial and academic, espousing their views on fuel choice; the economics of commercialization; the scope for increasing fuel cell vehicle performance; and a lengthy assessment of fuel cell electric versus battery electric drivetrain considerations, which reached the eventual shattering conclusion that there was room in the market place for both systems!

The oil companies were heavily represented, of course, although their individual presentations tended to be a little long-winded and repetitive.

Sjur Haugen of Norway's Statoil plodded laboriously towards presenting his company's conclusion—that methanol was the only viable fuel of the future. The majority of his industry contemporaries, however, apparently disagreed.

Esso UK is investigating the more cost-effective hydrogen route and, having dismissed central "merchant" production and supply methods, are deliberating the merits of either on-board reformers or local point-of-delivery generation using an on-site natural gas reformer utilizing steam, partial oxidation, or autothermal technologies. [Note: Toyota holds a recently-filed European Patent for a natural gas-based hydrogen reformer. For diagram and description, check out www.esp@cenet and search Patent No. EP 095 7063].

Shell also was hardly cagey about the fuel they regarded to be the ultimate long-term solution. Their introductory slide comprised a cartoon car and coffee pot and bore the legend "H₂ Fuel Cell Car: No More Polluting than an Electric Kettle."

None too surprising, perhaps, Shell does have a 16.33% share in Icelandic New Energy Limited, a company formed last year to harness geothermal energy for conversion to hydrogen.

Their representative, Dr. Roger Cracknell of Shell Global Solutions, stated that his company's objective was to make the hydrogen economy happen sooner. Shell didn't make light of the potential financial hazards involved. The requirement for standardization and risk-spreading industry/government partnerships were, he said, of paramount importance.

Shell was even in favor of applying pressure for change using fiscal inducements. Here, astonishingly, was an oilman actually proposing a carbon tax!



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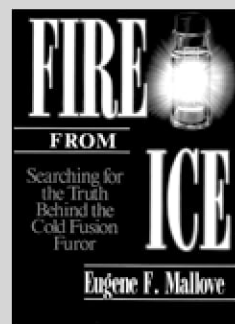


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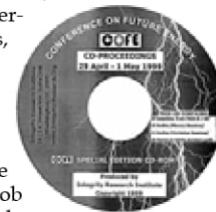
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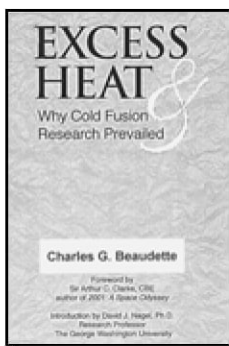
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